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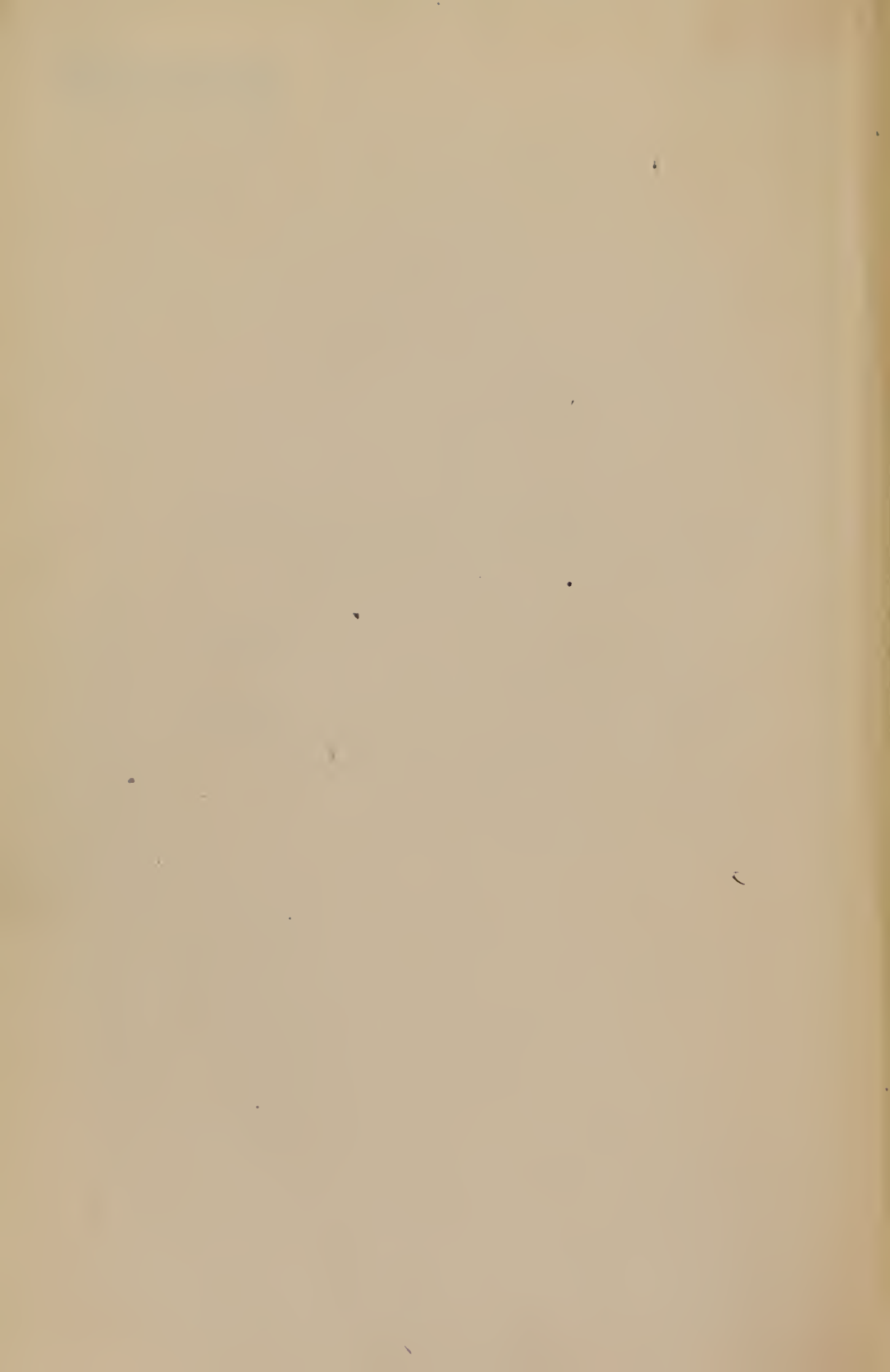
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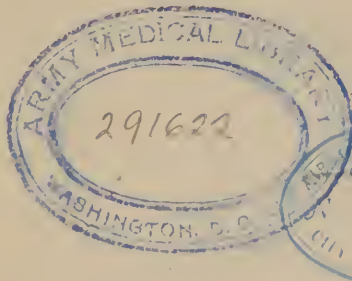
SCIENCE AND THE HEALING ART,

OR

A NEW BOOK ON OLD FACTS.

By JOHN CUSTIS DARBY, M. D.

MT. STERLING, KY.



LOUISVILLE, KY:

JOHN P. MORTON AND COMPANY.

1880

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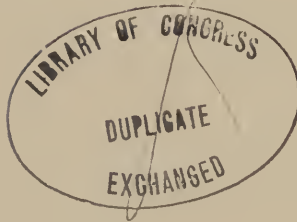
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BY JOHN CUSTIS DARBY.



PREFACE.

The larger part of this book was written over six years ago, at Lexington, Kentucky, where I practiced medicine for thirty years. Circumstances prevented its publication. I have not been able to see to read or write for over eighteen years; nor was I to command the services of an amanuensis when I wanted one. This will explain why there is some confusion in the arrangement of the chapters. Such as it is, I present it to the public.

THE AUTHOR.

MT. STERLING, KY., April 6, 1880.

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CHAPTER I.

PHYSICAL LAWS—DYNAMIC FORCES—ANIMAL LIFE.

The attraction of gravitation is supposed to be the expression of the law by which the heavenly bodies revolve in their orbits. It is, perhaps, the largest generalization of which the human mind can conceive, for it includes the universe. But the attraction of gravitation is inseparable from our conception of matter. Dynamic forces include in their definition the imponderables. Electricity, magnetism, heat, light, and cold belong to the imponderables; and it may be that dynamic forces have more to do with the revolutions of the heavenly bodies than the attraction of gravitation.

If light passes at the rate of one hundred and eighty-five thousand three hundred and fifty-eight miles in a second of time; and if it takes one hundred and twenty years for a ray of light to come from the nearest fixed star to our solar system; and if no one of the heavenly bodies ever revolves twice in the same orbit or path, it is impossible for the human mind to conceive how the attraction of gravitation, or a force depending upon or originating in ponderable bodies, is exerted at distances so incalculable.

Astronomy is the grandest, the most wonderful, and the most exact of all the sciences. Copernicus, Galileo, Kepler, Laplace, and Newton apprehended the general laws by which the celestial bodies floating in space are governed. They demonstrated those laws by proofs which are positive. Had they waited until Kirchhoff discovered the composition of the surface of the sun by

means of the spectroscope, Kepler's Laws, the Principia of Newton, and the *Méchanique Céleste* would never have been heard of.

The general laws controlling the formation of the strata of the earth's crust; the formation of continents, with their upheavals and depressions; the causes determining ocean tidal waves and atmospheric currents were apprehended by a Lyell and a Maury, as were the laws of astronomy by the men referred to above. Had the study of geology commenced in the chemical analysis of primary, secondary, and tertiary rocks; in the chemical analysis of drops of ocean-water and of grains of atmospheric air we should have no Lyell, no Maury. Astronomers and geologists studied matter as controlled by physical and dynamic laws. The existence of fossil remains, whether vegetable or animal, in coal-fields and limestone strata, is, in this consideration, but an accident. Men have disputed, as they will continue to dispute, whether matter had or had not an eternal existence, but there can be no dispute that there was a time when no living thing did exist or could exist on this planet. Life, then, had a beginning. The theory of Mr. Darwin is an hypothesis based upon hypotheses involving a postulate—*Give us life to begin with.*

We come now to consider the kingdom of living existences. Life and death are constant laws in this kingdom. Growth, the development of organs, and a limited period of longevity assigned to each family, whether vegetable or animal, are also constant laws. When either the flora or fauna die they return again to the kingdom which antedated vitality. As the astronomer occupies his mind with suns, stars, and planets; as the physical geographer occupies his mind with continents and oceans, with the zones, with solar heat, with terrestrial magnetism, with aerial and ocean currents, so should the physiologist occupy his mind in the study of life. He should begin with its appearance as most fully and most completely developed in either the vegetable or animal kingdom. In the one, they should consider life in the majestic oak; in the other, life in man as an Apollo Belvidere in form, with a large brain and large chest, a Napoleon in intellect.

They should consider life in woman as a Venus in beauty, a Minerva in intellect, and all that constitutes the wife and mother in purity, love, and affection. Can they discover the source of this intellect or the fountain of this love one whit more than the physical scientists can unravel the mysteries of the solar and stellar universe? The solar and stellar systems have ever been and will ever be governed by the same physical laws and dynamic forces. So has it been and so will it be on the planet which we inhabit.

The dual origin of life, the descent from parents, and the inheritance of physical, intellectual, and moral attributes and characteristics are as certainly fixed laws in the kingdom of vitality as are the attraction of gravitation and the revolutions of planets, suns, and stars within their orbits the laws of astronomy. Agassiz divides the animal kingdom into four subkingdoms: radiates, mollusks, articulates, and vertebrates. If I understand his propositions, he teaches that if the development theory of Mr. Darwin was the true theory of animal life the most perfect development of a radiate should be just below a mollusk, that the most perfect mollusk should be just below an articulate, that the most perfect articulate should be just below a vertebrate; but according to him such is not the fact. The cuttle-fish belongs to the class or family of radiates. It is one of the largest and most powerful of animals which swim in the seas. See Victor Hugo's *Toilers of the Sea*, and also a picture in one of the cathedrals in Spain where the cuttle-fish are upsetting a ship, when a saint rescues them. According to Agassiz, a humming-bird is not more like a lion or a shad-fish more like an elephant (all belonging to the class of vertebrates) than is a radiate like an articulate or a mollusk like a vertebrate; that they are different original existences; that they were created as they appear on this planet. The theory of Agassiz is that the flora and fauna, now appearing upon the earth, were created as we now see them. According to him men and all other animals were created as such. Cuvier, in his *Animal Kingdom*, considers the various forms of animal life, according to their sizes

and forms; according to the development of the organs within the cranium, the chest, and the abdomen, with their appurtenances of the brain and nerves; of the chest, with the lungs, heart, arteries, and veins, and with the abdomen, including the bowels with their vessels, venous and arterial, and with the limbs, arms, legs, etc. Cuvier described animals according to their outward and general forms—skeletons and organs of the animal body. Buckle, in his *History of Civilization*, says that Bichat stood next to Aristotle among great physiological thinkers. Bichat undertook to examine and to describe the tissues (he called them membranes) of the organs composing an animal body, about which Cuvier had written. Bichat taught that there were twenty-one tissues in the human organism, and he discovered the laws which govern many of these tissues. Since his day science has advanced. Virchow, the world's authority, according to scientists, has reduced the twenty-one tissues or membranes of Bichat to two—cells and intercellular substance; and when you come to examine Virchow's theory you find that the intercellular substance is itself composed of cells. The latest phase of this microscopical theory and demonstration is, according to Heitzmann, of New York, as published in the February number, 1879, of the *Louisville and Richmond Medical Journal*, as follows, viz: "In 1873 I published the results of my researches on protoplasm in the *Transactions of the Imperial Academy of Science of Vienna*, announcing that the protoplasm has a reticular structure. Each protoplasmic lump, in a certain stage of development, exhibits numerous conical threads, emanating from the nucleus and inosculating with the neighboring granules of the protoplasm. All granules are connected with each other by extremely delicate threads, so much so that the granules must be considered as the thickened points of intersection of a network which traverses the map of the protoplasm, and is in connection with a thin, uniform layer bounding the whole lump. I declared that the nucleus, the granules, the uniting threads, and the inclosing shell are formations of living matter, while the substance filling the meshes and vacuoles is a fluid, and as such devoid of

life." He objects to the cell theory of Virchow as incomplete and indefinite. I do not know what may hereafter be discovered. In the examination of animal life I start from his standpoint. Beginning as I do with the higher order of animals in the study of life, I affirm it to be an undeniable and incontrovertible fact that all life is of dual origin. No scientist disputes this fact till he gets so low down in the development of either vegetable or animal life as to be unable to tell, even with the aid of the most powerful microscope, by what laws their organization is governed. It might be said (and who could dispute the truth of the assertion?) that life begins in all the higher order of animals in the spermatozoon of the male and the egg of the female. They are living existences, but not capable of continued existence unless they come in contact and find a nidus. This is the birth of the fetus in utero. All else is growth. The spermatozoon and the egg referred to are made up of protoplasm or bioplasm, call it which you please. The physical, intellectual, and moral traits which distinguish the future man or woman were transmitted from their parents to this fetus in utero. After the birth of the fetus in utero all else is growth and development. A living healthful boy or girl is born, and grows to be a man or a woman. The dual origin of life involves and necessitates another physiological fact which is equally incontrovertible. It is that all life descends from parents, from parents who are fully developed and who have the capacity of propagating their kind. I have said that no one disputes the dual origin of life until he gets so low down in the scale that he can tell nothing about it. Were this not the law of animal life there would be no inheritance of either good or bad qualities, nothing could be gained from the pedigrees and the training of race-horses, and in a higher sense we could not account for moral and intellectual characteristics which sometimes pass through generations of men and women. Sometimes they skip from parents to grandchildren. Mr. Darwin and his disciples claim that the survival of the fittest is a law of animal life. If we suppose with him and his disciples that when life appeared on this planet it began in microscopic

germs or spores, and was afterward developed into the four subkingdoms of Agassiz, it is impossible for them to explain why myriads of living microscopic beings, with many that are visible to our eyes, have not ceased to exist hundreds of years ago. Mr. Darwin has laid great stress upon natural and sexual selection. His whole theory of evolution and development depends upon this law. It is a law of animal life, but we can not trace it so far back as we can the dual origin of life, or the descent of life from parent to child. Now these three laws, the dual origin, the descent from parents of certain characteristic traits of form and intellectual development, and the third law of natural and sexual selection, all show beyond a question of doubt that life does not begin even in bioplasm or in a cell; that it does not begin in the spermatozoon of the male or the egg of the female, but that it does and must begin in the life of the parents, and that the spermatozoon and egg are only material forms of its transmission. Were life developed from a microscopical living entity we could inherit nothing from our progenitors. If, according to them, there is only a missing link between men and monkeys, I would ask what did we inherit from them? As time with them is every thing, they must allow that several thousand years should make some little change. The architects of the pyramids of Egypt, the founders of the Brahmin and Buddhist religions, the author of the Pentateuch, the Homers, the Platos, the Aristotles, the Hippocrates, and Galens, of Greece, were a little nearer to the monkeys than we are.

Auguste Comte and John Stuart Mill, being judges of intellectual capacity, I ask, are there any men to-day who excel those worthies in mental power and moral grandeur? Life in us was life in them. When we consider life we must consider its highest capacities and its highest capabilities. Try as we may, we can not avoid or get around the necessity for a creator. I have said that there was a time when the kingdom of vitality was established on this planet; that prior to that time matter was governed by physical laws and dynamic forces; but these laws and forces continued, and though they were under the dominion of

the law of vitality, yet they influenced vital organisms; for instance, the temperature of vitality in man is about 98° F.; in a tree it is less. And again, all living beings, whether animal or vegetable, are influenced by atmospheric pressure. The zones in which either animals or vegetables exist, the altitude of mountains, the extent of plains, the ocean surroundings, atmospheric and ocean currents altogether influence human beings. They determine, to some extent, their forms and characteristics. And when we examine animal bodies we find that the laws of hydraulics have much to do with the circulation of the blood and other fluids within the body; that electrical and magnetic laws exert no less an influence and have a material system—the nervous system—for their manifestation. *Sunt multi homines sed pauci viri.*

In considering vitality I write of *virum*—the Moses of Africa, the Confucius of Asia, the Shakespeare of Europe, and the Washington of America, and not of the monkey-men of the Darwinians. The mind of an Aristotle, of an Archimedes, of a Galileo, of a Bacon, a Kepler, a Newton, a Laplace, a Morse, or a Barlow is not a correlation of forces nor the evolution of monads. Had Captain Eads gone to the headwaters of the Missouri and collected the little drops of water as they trickled from the rocks of the Sierra Nevada, or gathered from the bottom of the Mississippi the little grains of sand and analyzed them, he would have been as well prepared to know and to understand the hydraulic and atmospheric forces with which he would have to contend in building his bridge at St. Louis, or in constructing the jetties by which to deepen the channel of the great Father of Waters, as is the physiologist prepared to know or to discover the laws of the life of man by vivisection, by microscopical anatomy, or the study of the correlation of forces, or the evolution of monads. "The fool hath said in his heart there is no God" will be believed by mankind to be a truth when Mr. Darwin and every one of his disciples shall have been utterly forgotten.

CHAPTER II.

A NEW VIEW OF PHYSIOLOGY.

Dr. Elisha Bartlett, in his work on *The Philosophy of Medical Science*, says, "Our knowledge of physiology is not deducible from our knowledge of anatomy; our knowledge of pathology is not deducible from our knowledge of physiology." In the chapter on pathology he says, "In the preceding chapter I have endeavored to exhibit the independent nature of our knowledge of physiology. I propose in the present to treat in the same manner of pathology. I wish to show in the first place that our knowledge of the morbid processes and susceptibilities of the several organs and tissues of the body can not be inferred from our knowledge of their healthy processes. Pathology is not founded upon physiology. The latter is not the basis of the former; the one does not flow from the other; our knowledge of the one does not presuppose our knowledge of the other." Dr. Bartlett applies the same principles to therapeutics and the practice of medicine. In a review of this work published a short time after its appearance, in 1844, in the *Louisville Medical and Surgical Journal*, I said, "Dr. Bartlett reminds me of a boy lost in his father's forest. He knows every tree by name, an oak, an elm, a maple, a poplar, etc., but does not know how to find his way out of the forest."

It is to show the connections and relationships between some of the departments of medical science that the present volume is undertaken. When a child is born in perfect health, whose father and mother had been sound in mind and

body during their entire life, and grows to maturity having enjoyed perfect health, the law of constructive assimilation has alone been active. The living part of the bioplasm found in the embryo or fetus in utero continues to be developed into organs and to increase in size until we have a perfectly formed man or woman. The tissues composing the body of this man or woman continue to exist until death. The non-living parts of the several organs, like the non-living fluid which surrounds the threads in the bioplasm, are continually changing, but the original structure remains unchanged. It may be distorted by disease or accident, but that does not alter its identity. The laws of constructive assimilation and of destructive metamorphosis are like the tides—the ebbtide never begins to flow until the floodtide ceases, and so the floodtide until the ebbtide ceases.

Physiologists, I believe, all agree that effete matter was once a part of the living organism, and that it is separated from the tissues or organs by or through the laws of destructive metamorphosis. All effete matter is excrementitious, but all excrementitious matter is not, or was not, effete matter. Time was when it was said that an animal body was changed in seven years. Now science, so-called, declares that animal bodies are changed in twenty-one days. Dr. Draper, in his *Intellectual Development of Europe*, makes a digression to say that a ton and a half of food and drinks pass through the body of a man weighing one hundred and fifty or sixty pounds in one year, all having been a part of his living organized tissues. Dr. Thomas King Chambers, in his work called *the Renewal of Life*, refers to the Morgue in Paris, and says that a dead body there exposed does not undergo decomposition in so short a time as the body of a living person. As an illustration he presents a brick wall and says that we continually take out an old brick and put in a new one. The illustration is, I think, an unfortunate one, as this earth is not yet old enough to tell how long a brick will last. Dr. Dalton, in his learned work on *Physiology*, makes a more accurate calculation than Dr. Draper. He weighed the amount of food and drinks a man weighing say one

hundred and fifty pounds consumes in twenty-four hours, and then gives with equal accuracy the amount of excrementitious matter thrown from the body in twenty-four hours; all of which is, according to him, effete matter. A man weighing from one hundred and forty to one hundred and fifty pounds, who swallows seven pounds and a half of food and drink in twenty-four hours, will thus be entirely changed in from twenty-one to twenty-two days. The entire system of modern physiology is founded upon this doctrine. According to it, there is not an atom of matter in the body of a living man to-day which was in it one month ago. The food and drinks that we swallow are first mixed with the saliva, then with the gastric juice, and next with the pancreatic and intestinal juices and with the bile, if you please. By these intermixtures the food and drinks are changed into three forms. The oily portions of the food are changed into what is called fatty emulsion. The amylaceous forms of food are changed into glucose, and the nitrogenous forms of food are changed into albuminose. Ordinarily the fatty emulsion is all, or nearly all, taken up by the lacteals and conveyed to the mesenteric glands, and thence to the receptaculum chyli. There it is mixed with the lymph and passes through the thoracic duct to the vena cava descendens, and thence into the right side of the heart. The glucose and albuminose forms of food are taken up by the veins of the stomach and intestines and thence carried through the portal vein to the liver. From the liver they are carried through the hepatic veins to the vena cava ascendens, and through it to the right side of the heart. There meeting with the fatty emulsion they are mixed together and pass thence into the lungs. Passing through the lungs, they enter the left side of the heart through the pulmonary veins. When these three forms reach the left side of the heart they are called blood; indeed I do not know that I should follow these three forms of food as far as the left side of the heart. If they are not called blood as soon as they are absorbed from the stomach and intestines, physiological writers have taken no pains to say so. According to them the

three forms of food are like so many race-horses. They each and all rush to some particular part of the animal organism, there to become a part of the living tissues; or, if you please, to lay down a new brick in the place of an old one which has been removed. It will thus be seen that the roast beef and vegetables which we ate for dinner on Saturday are to-day (Monday) a part of our bodies. The practice of medicine, which the most distinguished writers profess to call scientific, is based upon this physiological doctrine.

According to Dr. Thomas King Chambers they have in the London hospitals night- as well as day-nurses, whose business it is to see that every patient drinks a certain quantity of milk or beef tea every three hours, and if the patient is asleep he has to be wakened. Science has forgotten Sancho Panza, who said, "Blessed is the man who invented sleep." Milk and beef tea are the mildest forms of diet, and this stuffing is to put a new brick into the place of an old one, which, according to science, is continually passing out of the wall. This practice is not confined to London hospitals. It may be modified, but it is pursued by the scientific physicians of both Europe and the United States.

I teach that when a man is subjected to starvation or is sick, then the law of destructive metamorphosis begins in his body, and that while this law continues to operate the law of constructive assimilation is suspended; that so long as the starvation or sickness continues no part of any of the three forms of food absorbed from the stomach or intestines becomes a part of the living organism; that when the starvation or sickness ceases and the individual obtains food, or begins to get well, then the law of constructive assimilation begins to operate, and that then the three forms of food are ultimately changed into living organized forms. When the three forms of food spoken of are absorbed, I say that the conversion of the food into blood has then hardly begun. I do not say that patients require no food during fevers; but this is not the place for the consideration of that question. I teach that every gland in the body, except the

reproductive glands, is exclusively and entirely engaged in the conversion of the food into blood preparatory to the formation of tissues from the blood. I here speak of tissues in the sense of Bichat.

I will now endeavor to show how the food is converted into blood. I have said that the oily portion of the food, or fatty emulsion, is conveyed by the lacteals to the thoracic duct, and thence to the right side of the heart. In its passage through the mesenteric glands it is subjected to certain vital changes. Virchow says he saw white corpuscles as they came out from those glands. They are formed or generated by other lymphatic glands. The two forms of food which pass to the liver through the portal vein are there subjected to certain vital changes necessary to their becoming constituent parts of the blood. Bichat observed, in the early part of this century, that the bile begins to be poured into the duodenum almost immediately after digestion commences. I teach that this bile, the clear and comparatively sweet bile, is an excrementitious substance formed from glucose and albuminose forms of food as they pass through the liver; that this bile is a substance necessarily separated from these forms of food in their metamorphosis into the constituent parts of the blood. Physiologists teach that two pounds and a half of bile are ordinarily secreted by the liver of a healthy man during every twenty-four hours. I say that the greater part of this bile is formed from the food as it passes through the liver. There is another form of bile—the dark, viscid, bitter bile. I hold that this form of bile is formed from effete matter brought from the abdominal organs in its return to the right side of the heart. It is not denied that all of this blood has to pass through the liver before it reaches the vena cava ascendens. The conversion of effete matter into bile is effected by the liver for the most part after the forms of food referred to have passed through the liver. The formation of bile from effete matter will be considered in another place. I teach that the larger part of the bile in health is formed from the food as it passes through the liver, and that a lesser part is formed from effete blood as it passes

through the liver. No physiologist teaches that bile is thus formed or that it is formed from the elements and substances out of which I say it is made. Dr. Murchison, in his valuable little work on Functional Derangements of the Liver, says that the bile is formed from the albumen in the blood. I believe that physiologists generally agree with him. Not one of them, so far as I know, has ever taught that a portion of the bile, and, as I believe, a large portion, is formed from the food as it passes through the liver, and they all deny that any part of the bile is formed from effete matter. Effete matter is, according to them, separated from the blood and thrown off from the body by two organs—the lungs and the kidneys, but particularly by the latter. I believe they also teach that effete matter is to some extent thrown off by the skin. Were they to allow that the bile is formed from effete matter they would be compelled to admit that the bile is an excrementitious substance, because they all teach, and they are compelled to teach, that effete matter is a substance which has to pass out of the body. This denial that the bile is formed from effete matter involves them in the assumption that the bile is an incrementitious and not an excrementitious substance, and also involves them in the other assumption that the bile is continually reabsorbed. There is some little qualification as to the reabsorption of the bile. Cholesterin, one of the fractional parts of bile, is, they say, converted into stercorin, and thus passes out of the body. The general assumption that the bile is for the most part reabsorbed, and that it is not entirely and altogether an excrementitious substance, involves them in a dilemma. To answer this difficulty, Claude Bernard had discovered, and the so-called scientific medical world is in acclamation at his discovery, that the liver possesses glycogenic functions, and that *it is* the special function of the liver. Sugar, under some of its forms, is said to be sometimes found in the blood of the hepatic veins. I think it is more than doubtful that it ever was there found when the animal, the blood of which was examined, was in *perfect health*. Bichat said that physiological laws could not be discovered when the body of the animal ex-

perimented on was in torture. I agree with him. It is the function of the liver to change the two forms of food, glucose and albuminose, coming to it from the stomach and intestines into something nearer akin to blood. When we consider how large a portion of our food is of a saccharine composition, is it strange that the liver is sometimes unable to change it, and that it passes into the hepatic veins as sugar? Ordinarily the fatty emulsion coming up through the thoracic duct to the right side of the heart is so changed in its passage through the lungs that it does not appear as fatty emulsion in the blood of the carotid artery, but sometimes it does there so appear. In the same manner sugar sometimes passes through the liver and is found in the blood of the hepatic veins. But it is said that sugar is found in the hepatic veins of animals which feed entirely on flesh. This may be a fact. I doubt it. To make the most of Claude Bernard's discovery, all that can be said of it is that when the liver acts upon the glucose and albuminose which come to it intimately mixed together, it separates from them substances out of which it makes the sweet bile, and that, in that separation and vital action upon these forms of food, one of them, the glucose, may be changed into glycogen.

I hold that one of the functions of the liver is to change the food into something nearer akin to the constituent parts of the blood. These two forms of food, having thus been acted upon by the liver, pass on to the right side of the heart. They there meet with the fatty emulsion which had already been subjected to the action of the mesenteric glands. These all mixed together with a great quantity of lymph pass directly to the lungs, where they are acted upon by the inspired air. It is supposed by physiologists generally, I believe, that some portion of the food is consumed as it passes through the lungs, in the production or generation of animal heat. When these three forms of food reach the left side of the heart, having already been subjected to the vitalizing action of the liver, of the mesenteric glands, and of the lungs (for in this sense I consider the lungs to be glands), they are distributed to all parts of the body. They

have yet, however, to be subjected to the action of the other glands before they can be called blood. The lymphatics, like the veins, originate in all parts of the body. In a physiological sense the lymphatics separate from the blood in the arterioles certain parts of the food which they carry to the lymphatic glands, where it undergoes another vital action. From these glands it passes on to the thoracic duct. Lymph corpuscles are found in large numbers in the lymph as it is about to pass into the right side of the heart. When these corpuscles have passed through the lungs and on to the left side of the heart, they no longer appear in the blood of the carotid artery, for instance, except in the proportion of one white corpuscle to three hundred red ones, which, according to Virchow, is their normal proportion in the arterial blood of a man in perfect health. They become red blood corpuscles in their passage through the lungs.

Considering the size of the kidneys a very large quantity of arterial blood is sent to them. For what purpose is it sent? This arterial blood, sent to those organs, contains three forms of food which is to be subjected to another elimination in the kidneys. They have to separate a large portion of the liquids with which the food had been mixed up. I refer to the saliva, the gastric juice, the pancreatic, and intestinal juices. They also separate certain solid constituents, all of which are poured into the bladder. Recent physiological facts have shown that the purest blood found in the body is that coming immediately from the kidneys. It is because the food, under its three forms, had been subjected to one of its last eliminations in its passage through those organs. The renal veins convey to the vena cava this pure blood. The glands of the skin have also to perform their part in the conversion of the food into blood. This they do in the elimination of watery parts, which they throw off as insensible perspiration.

This is the round of the changes and glandular actions to which the food is subjected before it becomes blood, or can properly be called blood; but it has to be subjected to another large

gland. I refer to the spleen. The blood which comes from the spleen has a larger amount of fibrin in it than had the blood which was conveyed to it through the splenic artery. I believe it also contains a larger number of white blood corpuscles. The spleen is continually pouring into the portal vein rejuvenated blood. It is out of the blood thus prepared or made, as I have so fully stated, that the tissues are formed. This formation, or constructive assimilation, goes on from the beginning of life until the man or woman has arrived at a mature growth. During all this time the excrementitious matter thrown out from the body was separated from the food. I affirm that no effete matter was separated from the tissues during all this time if the individual continued in perfect health. After the individual has arrived at mature growth, the food which has up to this time been formed into the living tissues, is now used in maintaining the strength, the animal heat, and all other functions, without being metamorphosed into living organized tissues or parts of the body. It is then, and then only, that the excrementitious matter corresponds so nearly in weight with the amount of food and drinks swallowed. When a growing child is gaining a pound a day in weight, there can be no such correspondence. Indeed some physiological writers maintain that pigs sometimes increase in weight beyond the amount of food and drinks consumed. The notion that destructive metamorphosis is continually going on in healthy animals, thereby separating from the tissues effete matter, involves another difficulty. What becomes of this effete matter? A large part of it, they say, is separated from the blood by the kidneys. I teach that neither effete matter nor effete blood ever reaches the left side of the heart. When it is separated from the tissues and emptied into the veins it is removed from the blood by the liver, forming out of it the viscid, dark, bitter bile. The liver takes from the blood all effete matter brought to that gland through the portal vein. It changes it into bile, which is an excrementitious substance. The lungs also separate effete matter, when there is any, from the blood, and throw it off as carbonic acid. When there is no effete matter the lungs throw

off effete blood as it passes through them. When the blood is not converted into tissues it becomes itself effete, and has to be thrown off from the body by the liver and lungs. But physiologists teach that effete matter, which is continually being separated from the tissues, is formed into urea, cholesterin, etc.

Dr. Murchison, in the work already referred to, says that one of the functions of the liver is the production of urea, and that sometimes the liver generates or makes uric acid or the urates. Where glands or organs are provided for every distinct function, as in the bodies of the more fully developed animals, it is contrary to the uniform simplicity of nature for any one gland to perform three or four functions entirely different. In pathological conditions glands may act vicariously for each other, but it is not so in perfect health. It is the especial function of the kidneys to separate certain parts from the food which they form into urea, and every other thing found in the bladder. They are as much organs engaged in sanguification as are the salivary glands, the stomach, the liver, and the lungs. If the liver formed urea out of any part of the blood, that urea ought to be immediately put into a channel which would remove it from the body. It ought to pass from the liver along with the bile and be poured into the duodenum. But physiologists teach that the urea formed by the liver passes through the hepatic veins into the vena cava. A poison thus formed is sent into the general circulation to be distributed to all parts of the body. If it were possible to conceive that urea is ever poured into the vena cava of the healthy man, it ought to be separated from the blood as it passes through the lungs, and thrown out of the body along with the expired air. But such is not the doctrine of the schools. They teach that urea and several other poisons enter the left side of the heart and are distributed to every part of the body. I say they teach this unless they suppose that urea and other poisons are taken by some elective affinity through the renal arterics to the kidneys, there to be separated, as they say, from the blood. The whole thing is an absurdity.

The above outline of the doctrines which I propose to discuss in this book is taken in part from an article I wrote for the *St. Louis Medical and Surgical Journal*, of February, 1878. Let the reader bear in mind that in this chapter I have considered physiological and not pathological conditions. This applies particularly to what I have said as to effete matter and the offices of the lymphatic vessels.

CHAPTER III.

WRITERS ON PHYSIOLOGY.

As some persons may read this book who are not physicians, we will quote occasionally from medical authorities, so that they may know what is taught in the schools. And first we will quote a few extracts from two of the most distinguished medical authorities in the United States touching questions alluded to in the foregoing chapter.

Dr. Dalton, in his work on *Physiology*, says, "We can now estimate from the foregoing details the entire quantity of material assimilated and decomposed by the living body. For we have already seen how much food is taken into the alimentary canal and absorbed by the blood after digestion, and how much oxygen is appropriated from the atmosphere during the process of respiration. We have also learned the amount of carbonic acid evolved with the breath and that of various excretory substances discharged from the body. The following table shows the absolute quantity of these different ingesta and egesta, compiled from direct experiments which have already been given in the foregoing pages:

"ABSORBED DURING TWENTY-FOUR HOURS.

"Oxygen,	1.019 pounds.	
Water,	4.735	"
Albuminous matter,396	"
Starch,660	"
Fat,220	"
Salts,040	"
Total,	<u>7.070</u>	"

"DISCHARGED DURING TWENTY-FOUR HOURS.

"Carbonic acid,	.	.	.	1.535	pounds.
Aqueous vapor,	.	.	.	1.155	"
Perspiration,	.	.	.	1.930	"
Water of the urine,	.	.	.	2.020	"
Urea and salts,110	"
Feces,320	"
Total,	.	.	.	7.070	"

"Rather more than seven pounds are therefore absorbed and discharged by the healthy adult human subject; and, for a man having the average weight of one hundred and forty pounds, a quantity of material equal to the weight of the entire body thus passes through the system in the course of twenty hours. It is evident also that this is not a simple phenomenon of the passage or filtration of foreign substances through the animal frame. The materials which are absorbed actually combine with the tissues and form a part of this substance, and it is only after undergoing subsequent decomposition that they finally make their appearance in the excretions. None of the solid ingredients of the food are discharged under their own form in the urine, viz. as starch, fat, or albumen; but they are replaced by urea and other crystallizable substances of a different nature. Even the carbonic acid exhaled by the breath, experience has taught us, is not produced by a direct oxidation of carbon, but originates by a process of decomposition throughout the tissues of the body somewhat similar to that by which it is generated in the decomposition of sugar by fermentation. These phenomena, therefore, indicate an actual change in the substance of which the body is composed, and shows that its entire ingredients are incessantly renewed under the influence of the vital operations.

"From experiments while living on an exclusive diet of bread, fresh meat, and butter, with coffee and water for drinks, we have found that the entire quantity of food required during twenty-four hours, by a man in full health and taking free exercise in the open air, is as follows, viz:

“Meat,	16 ounces, or 1.00 lbs. avoirdupois.
Bread,	19 “ 1.19 “ “
Butter and fat, $3\frac{1}{2}$ “	.22 “ “
Water,	52 fluid oz. or 3.38 “ “

“That is to say, rather less than two and one half pounds of solid food and rather over three pints of liquid food. Of course the quantity varies according to circumstances, the kind and quantity of the food, and the constitution of the individual.”

After a careful examination of Flint's Physiology, I can not find that he differs from Dalton as to the positions taken by the latter in the above extract. Dr. Draper certainly agrees with him. Now if in an animal body the ingesta and egesta, including the oxygen inspired and the carbonic acid exhaled, exactly balance each other, I confess that it is beyond my comprehension to see how a child or a pig could grow or increase in weight. I conjecture that the experiment made by Dr. Dalton was on a fully grown individual whose weight remained stationary. I alluded in the last chapter to a fact, given I think by a German physiologist, that pigs sometimes increase daily more in weight than is contained in the food and drinks swallowed by them. The fact may be true; its explanation must be conjectural. Ordinarily we know that young and growing animals, including man, gain but a small proportion in weight daily compared with what they eat and drink. I differ totally from Dr. Dalton in his supposition that during healthful growth any part of the organized forms in a living body is separated from it and becomes effete matter. How the excreta to which he refers are formed, and how they escape from the body, I will notice in another connection.

Dr. Flint says, “Assuming the daily allowance of bread to be nineteen ounces, and estimating the allowance of meat to be sixteen ounces, and the quantity of saliva secreted during twenty-two hours of interval, the entire quantity in twenty-four hours would amount to twenty thousand one hundred and sixty-four grains, or a little less than three pounds avoirdupois, of which rather more than one half is secreted during the intervals of eat-

ing. All that portion of saliva secreted while we are masticating our food is mixed more or less intimately with it and passes into the stomach and intestines and is again taken up by the venous absorbents. This saliva is secreted from arterial blood. This first addition to the food is the beginning of the change in it from dead matter into living blood." As to the gastric juice, the next addition to the food, Dr. Flint says, "Estimates, therefore, like Bidder's and Schmidt's, who put the quantity of gastric juice secreted in twenty-four hours by a healthy man of ordinary size at six thousand four hundred grams, or about fourteen pounds, are probably not exaggerated, though of necessity they are merely approximative. This estimate for quantity is a very high one, but the authorities for it stand in the first rank of physiologists."

That part of the food which is prepared by the stomach for absorption, by its admixture and union with the saliva and gastric juice, and which is taken up by the venous absorbents of the stomach, passes directly to the liver. The remainder of the contents of the stomach passes into the duodenum. The next addition to the food is multiple. As it passes out of the stomach into the duodenum it begins to be mixed with the pancreatic juice, with the bile, and with the intestinal juices. The intermixture with the latter continues throughout the small intestines. The quantity of the intestinal juice is supposed by physiologists to be very large, perhaps more than the saliva and gastric juice combined. Dr. Flint says, "There is no accurate basis for an estimate of the quantity of pancreatic juice secreted in the twenty-four hours in the human subject or of the quantity necessary for the digestion of a definite amount of food."

The composition of the human bile is as follows:

Water,	915.00 to 819.00
Taurocholate, or choleate of soda ($\text{NaOC}_{52}\text{H}_{45}\text{NO}_{14}\text{S}_2$),	56.50 to 106.00
Glycocholate, or choleate of soda ($\text{NaOC}_{52}\text{H}_{42}\text{NO}_{11}$),	traces
Cholesterin ($\text{C}_{25}\text{H}_{22}\text{O}$),	1.00 to 2.66

Biliverdin,	14.00 to 30.00
Lecithin, margarin, olein, and traces of soaps,	3.00 to 31.00
Choline ($C_{10}H_{13}NO_2$),	traces
Chloride of sodium,	2.77 to 3.50
Phosphate of soda,	1.60 to 2.50
Phosphate of potassa,	0.75 to 1.50
Phosphate of lime,	0.50 to 1.35
Phosphate of magnesia,	0.45 to 0.80
Salts of iron,	0.15 to 0.30
Salts of manganese,	traces to 0.12
Silicic acid,	0.03 to 0.06 .
Mucosin,	traces
Loss,	3.45 to 1.21
	<hr/> 1000.00 1000.00

I think that some physiologists estimate the quantity to be several pints, perhaps more. Physiologists generally estimate the quantity of bile secreted in twenty-four hours in a man of ordinary size to be two and a half pounds. It will be seen that the food and drinks are intermixed with fluids, the saliva, the gastric, intestinal, and pancreatic juices, which exceed them in quantity three or four, perhaps six or eight, times; for physiologists do not agree with any accuracy as to the quantity of these juices secreted. It is a noteworthy fact that they are all secreted from arterial blood. The bile, on the other hand, is secreted from venous blood, or rather, I should say, from a fluid brought to the liver through the portal vein. Physiologists all agree, I believe, that when glands are actively secreting there is a suspension of the process of disassimilation in their organisms; that is to say, no effete matter is separated from the tissues of a gland while it is secreting actively, or secreting at all perhaps. The saliva, gastric and other juices named above do not contain effete matter, except a fractional part of epithelium. These fluids are all intermixed with the food to prepare it for absorption by the veins and lacteals; to prepare it, the food, for its transformation into blood. They are absorbed, it is true, along with the food—that is, the greater part of them; but as they are

each and all of them secreted at and during every meal we must suppose that those secreted yesterday must escape from the body within a given time.

Now I suppose (for I do not know that the question has ever been accurately considered) that the escape of water and aqueous vapor from the body through the kidneys, lungs, and skin will not correspond exactly with the quantity of water and other fluids drunk and with the quantities of saliva, gastric, and other juices secreted. I therefore suppose that a large proportion of the two and a half pounds of bile secreted daily and poured out into the duodenum is what is allotted to the liver to throw off from the body of these fluids. If I am correct in this opinion then the bile is *quoad hoc* an excrementitious fluid. If the separation of effete matter is in abeyance while glands are secreting, the veins going from the stomach and intestines to the liver during digestion must be filled almost altogether by the food and the juices with which it is intermixed. In this connection I am only considering the functions of the liver as an organ of sanguification.

For the benefit of the general reader I will here give an outline of the circulation of the blood.

The circulation of the blood in the human body is carried on through four systems—the systemic, the pulmonary, the capillary, and the portal system.

The *systemic* is divided into two—the arterial and the venous. By the arterial system the blood is carried from the left ventricle of the heart through the aorta and arteries to all parts of the body. The arteries have no valves in them. By the venous system the blood is carried from all parts of the body to the right auricle and ventricle of the heart. The veins distributed through the superficial parts of the body, including the limbs and all muscular parts, have valves in them. By the venous system the blood is carried from all parts of the body to the right auricle or ventricle of the heart. Dr. Austin Flint says, "A great discovery preparatory to that of the circulation was made by Fabricius ab Aquapendente, professor at

Padua, who, in the words of Flourens, had a double glory: 'He discovered the valves of the veins and was the master of Harvey.' This was demonstrated to Harvey at Padua, though Fabricius does not appear to have had any idea of their functions. It is possible that this anatomical fact may have directed the mind of Harvey in his first speculations on the circulation. Harvey observed the knots in the veins of the arm when a ligature is applied, as for phlebotomy, and showed that the spaces between these knots which are formed by the valves could be emptied of blood by pressing toward the heart, and would not fill with blood while the finger was kept at the lower extremity. It was impossible by pressure with the fingers to force the blood back through one of the valves."

Notwithstanding the very high estimate thus expressed by Dr. Flint of our knowledge of the existence of valves in the veins and their offices, very little importance has been attached by medical men to the uses of the valves in their explanations of the phenomena of disease. We will refer more particularly to them hereafter, and point out in what parts of the venous system they are absent.

The *capillary* system lies intermediate between the extremities of the arteries or arterioles and the microscopical rivulets which are the beginnings of the veins. It is here that assimilation or constructive metamorphosis is effected; in other words, it is in the capillary system that the materials of the blood are converted into living tissue. It is in the capillaries also that disassimilation or destructive metamorphosis is effected. Here carbonic acid is liberated or generated and passes with other effete matter into the venous blood, causing it to become dark in color. The capillary system is distributed through all parts of the body. But we must call attention to the circulation of the blood in the capillaries of the glands. When the parotid or mammary glands are secreting actively, the blood passing through them does not become darker in color in the veins, as is usually the case; in other words, the separation of effete matter seems to be suspended while the secretory functions of the glands are being

performed. This fact has been observed by all recent physiologists. Constructive and destructive metamorphosis seem to be suspended while active secretion is going on in a gland. The blood returning from a gland through the veins is almost as red as that which enters it through the arteries. I desire the reader to pay particular attention to this fact. The whole volume of blood from all parts of the body is returned to the right side of the heart through the ascending and descending vena cava, which empty into the right auricle of the heart. The capillary system is an intermediate division which lies between the arteries and veins.

Third. The *pulmonary* system is peculiar in this, that the venous blood is carried from the right ventricle to the lungs by an artery—the pulmonary artery. After throwing off carbonic acid and absorbing oxygen in the lungs, blood is said to be aerated, that is, it becomes arterial, and is returned through the veins—the four pulmonary veins—to the left auricle and ventricle. It will be observed that in both heart and lungs the whole volume of blood passes immediately through each. These two organs never rest. The suspension of action in either causes instant death. Such a law does not apply to the capillary system; that is to say, the circulation may be suspended for a time in parts of the capillary system without causing any serious difficulty. The lungs are supplied by an independent system of blood-vessels for their nutrition. I refer to the bronchial arteries and veins, which are entirely independent of the great current of blood passing through those organs.

Fourth. The *portal* system. Arterial blood is conveyed, as elsewhere, to all the abdominal organs; but the venous blood, returning from the stomach, the whole of the intestines and their appendages, from the pancreas and spleen, does not go immediately back to the heart, as it does from every other part of the body, but is emptied into the great portal vein, which pours its contents into the liver. The portal vein and its branches have no valves in them. Intermediate or between the arteries which go to supply the organs just named and the veins

returning the blood from them lies their division of the capillary system. Every one at all acquainted with comparative anatomy knows that the lungs and the liver are supplementary organs; that in animals—mollusks, for instance—where the livers are enormously large in comparison with the size of their bodies, the breathing apparatus is correspondingly small, and that in animals where the breathing apparatus is very large in comparison with the size of their bodies the livers are in them also correspondingly small. The portal circulation is unlike that of any other part of the animal organism—that is, in man. Arterial blood is carried to the liver through the hepatic artery, which is very small in comparison with that gland; very much smaller in comparison with the liver than are the renal and splenic arteries in comparison with the kidneys and spleen. Except in the glands which secrete, the blood, which is conveyed to every part of the body—to the brain, to the muscles, bones, etc.—is intended to maintain the integrity of those organs and to enable them to perform their functions, whatever they may be; and it also goes to repair wasted tissues, when there is any waste, and to increase the size of organs in growing animals. I maintain, and I do not see how any physiologist can dispute the fact, that the blood conveyed to the liver through the hepatic artery is for the exclusive purpose of maintaining the integrity and vitality of that organ, and to enable it to perform its functions; that the office of the bronchial arteries and of the hepatic artery is one and the same.

Then we must inquire why so large an amount of blood is conveyed to the liver through the portal vein. We have seen that part of the blood found in the portal vein comes from organs which had been engaged in secreting certain juices, as the gastric, pancreatic, etc. The secretion of those juices from arterial blood, brought to the secreting glands or organs, left the venous blood flowing from them in an impoverished or effete condition. This venous blood is conveyed to the liver, there to be acted upon for some purpose. I maintain that that purpose is to form bile out of it, the dark, viscid bile; but I will not

consider this question at this time. The other part of the fluid conveyed to the liver through the portal vein is the food which has been absorbed from the stomach and intestines. The older physiologists of modern times taught that after the food was conveyed to the stomach it was converted into chyme; that after it passed out of the stomach into the duodenum it was changed into chyle; and that this chyle was all absorbed by the lacteals and conveyed through the mesenteric glands to the thoracic duct. Magendie was, I believe, among the first of modern physiologists to show that the food was not all absorbed by the lacteals, and that a large portion of it was absorbed by the veins in the stomach and intestines. Dr. Murchison says, in the work to which I have already referred, that Galen, one of the earliest and most distinguished of the Greek physicians, entertained the same opinion. How he arrived at that knowledge or conclusion I leave for those physiologists to determine who maintain that we can not ascertain the laws of life without the microscope and visible experiments made on living animals. My attention was first called to what may be called a demonstration of this venous absorption by an article, a monograph of M. Guersant, of Paris, on Carreau, the Cachexia Africana of the United States. He gives the instance of a man who was executed. To every appearance he was well and robust. He was, at any rate, corpulent. The autopsy showed that the mesenteric glands were completely degenerated; they were extraordinarily large and of a cheesy consistence. In that man's case all the food, including fats and oils, must have been taken up by the venous absorbents and carried to the liver. I suppose that the fact is not now disputed that that portion of the food principally which is composed of bread, rice, fruits, and vegetables, and that composed of meats, are absorbed by the veins and conveyed through the portal vein to the liver. Physiologists differ, I believe, as to the amount of fatty emulsion taken up by these venous absorbents; and also whether the lacteals do not convey to the thoracic duct some portion of one or both of the other two forms of food. The question, it seems to me, ought to arise in the mind of every thinking

physiological investigator, why so large an amount of the food absorbed should pass through the liver before it enters the general circulation? I have expressed the opinion in the second chapter that the bile, which begins to be poured out into the duodenum almost immediately after the food reaches the stomach, is formed from the food and drinks and the juices with which they are intermixed; and that the two pounds and a half of bile which are poured out from the liver into the duodenum every twenty-four hours in a man of ordinary size is thus to be accounted for. This applies to a condition of perfect health. The formation of bile from effete matter will not here be considered. This opinion I claim to be original with me. The older physiologists, I believe, did not dispute the fact that bile was formed from effete matter. But as all physiologists who now claim to be scientific investigators deny both of my propositions, it remained for M. Claude Bernard, in 1848, to discover that the liver possessed glycogenic functions, and that indeed the sugar-making processes going on in the liver constituted its principal function. This discovery was arrived at by means of the microscope and vivisection. It has also been discovered, according to the same scientific investigators, that urea is another one of the substances generated or produced in the liver of a perfectly healthy man. I will notice this last in another place. The bile not being formed from the food and its intermixtures, nor from effete matter, according to the highest scientific authorities, we ask, from what is it formed? They say that it is formed from albumen, one of the constituent parts of healthy blood. When we ask again, what becomes of it (the bile) and what are its uses, we are told that it is poured into the duodenum to be mixed with the food to prepare it for absorption, and that it is all or nearly all reabsorbed along with the food and the juices with which it is intermixed. Cholesterin, a fractional, crystallizable substance found in the bile, is changed, they say, into stercorin, and passes off with the feces. Physiologists do not deny that not a single constituent part of the bile can be found in the blood of the portal vein of a healthy man; and yet the knowledge of that

fact does not prevent them from maintaining that the bile, with the exception referred to, is all reabsorbed. No physiological writer has ever claimed that the saliva, after it is poured out into the mouth, ever returns to the salivary glands, nor that the gastric juice and the pancreatic and intestinal juices are ever returned to the glands secreting them. After they are secreted they are permitted to pass out of the body through the kidneys, skin, and lungs. When it is considered how large a proportion of the food consumed by individuals living in tropical and temperate climates is composed of substances convertible into glucose, one of the forms of sugar, we say it is not strange that sugar should sometimes be found in the blood of the hepatic veins which empty it into the vena cava. We have referred to the fact that the lungs and liver have corresponding duties to perform.

Dr. Dalton, in his *Physiology*, page 157, says, "The fatty substances taken up by the portal vein, like those absorbed by the lacteals, do not at once enter the general circulation, but pass first through the capillary system of the liver, thence they are carried with the blood of the hepatic veins to the right side of the heart, and subsequently through the capillary system of the lungs. During the passing they become altered in character, as above described, and lose for the most part the distinguishing characteristics of oily matter before they have passed beyond the pulmonary circulation. But as digestion proceeds an increasing quantity of fatty matter finds its way by these two passages into the blood; and a time at last arrives when the whole of the fat so introduced into the blood is not destroyed in its passage through the lungs. Its absorption at this time taking place more rapidly than its decomposition, it begins to appear in moderate quantity in the blood of the general circulation; and lastly, when the intestinal absorption is at its point of greatest activity, it is found in considerable abundance throughout the entire vascular system. At this period, some hours after the ingestion of food rich in oleaginous matter, the blood of the general circulation every where contains an abundance of fat derived from the

digestive processes. If blood be then drawn from the veins or arteries in any part of the body, it will present the peculiar appearance known as chylores, or milky blood. After the separation of the clot the serum presents a milky appearance, and the fatty substances which it contains rise to the top after a few hours and cover its surface with a partially opaque and creamy-looking pellicle. This appearance has occasionally been observed in the human subject, particularly in bleeding in apoplectic attacks occurring after a full meal, and has been mistaken in some instances for a morbid phenomenon. It is, however, a perfectly natural one, and depends simply on the rapid absorption at certain periods of digestion of oleaginous substance from the intestines. It can be produced at will at any time in the day by feeding him with fat meat and drawing blood six or eight hours afterward from the carotid artery or jugular vein. This state of things continues for a varying length of time, according to the amount of oleaginous matter contained in the food."

CHAPTER IV.

THE LYMPHATIC SYSTEM.

It will be seen from the extract given at the conclusion of the last chapter that ordinarily oils and fats coming from the right side of the heart through the pulmonary artery disappear in their passage through the lungs, and are not to be discovered in the blood of the carotid artery, and that this condition of things sometimes exists in health. Ordinarily all that portion of the food which is converted into glucose before it is absorbed and carried to the liver is so changed by its passage through that gland that sugar is not found in the hepatic veins; but when it is considered how very large an amount of this form of food passes through the liver, are we not permitted to suppose that the occurrence of sugar in the blood of the hepatic veins and ascending vena cava is a condition exactly similar to the occurrence of oils and fats in the blood of the carotid and other arteries? Dr. Dalton says that these oils and fats may be seen even in blood drawn from the veins. In both instances the oils and fats and the sugar found in the blood have to go the round of the circulation, perhaps many times, before they disappear or are completely changed.

Physiologists speak of the intervals of digestion. Properly speaking, there are no intervals of digestion. I here mean by digestion the conversion of food into blood. After we take our daily meals there is a time, longer or shorter, varying according to conditions not here to be named, during which all the food is absorbed or removed from the stomach and intestinal canals. In

that sense there are intervals, but after the food has entered the veins and lacteals, digestion proper or the conversion of food into blood has just fairly commenced. Trees and plants convert inorganic matter into organic living forms. It is the labor of their lives. All animals convert what we call food, whether it be grasses, grain, fruit, or meat, into blood, and blood into living tissue. In trees the wood of which the bodies consist remains unchanged and continues to increase from year to year, so that the rings of annual growth show unmistakably the age of the tree. There are trees a hundred, perhaps a thousand, years old. It is because, unlike the tides, physiologists suppose that constructive assimilation or the formations of tissues (in the sense of Bichat), and disassimilation or the separation of effete matter from the tissues, go on at one and the same time in an animal body; that animal bodies are completely renewed, according to Drs. Draper, Dalton, and others, in twenty days.

There is no explanation of hereditary and inherited diseases; there is no explanation of a man being bitten by a maddog to-day and having hydrophobia seven years afterward; there is no explanation of insanity where a man or woman becomes insane and continues in that condition from one to ten or more years, who is again restored to perfect health, both bodily and mental, and whose memory treasures up all forms of knowledge acquired up to the time he or she became insane, if the doctrine of constructive and destructive metamorphosis of the whole organism is effected every twenty days or every twenty months be true, as taught in all the schools.

Many years ago I was informed by a gentleman in Virginia, engaged in a herring fishery, that sometimes they caught more fish than they could salt and pack before they would spoil; that they hauled out these surplus fish with all the fish offal into the fields and fed them to their hogs; and that the flesh of the hogs so fed, when made into pork and bacon, tasted so strong of fish that many persons refused to eat it. The fish were caught, I believe, in March and April, and yet the meat of the hogs which fed upon them, when killed six or eight months after-

ward, tasted of fish. According to all modern physiologists the bodies of these hogs had been changed many times in those six or eight months.

No physiologist who allows himself to think can suppose that the food is converted into living organized forms before it has been changed into blood. In the blood is to be found every element of which the animal body is composed. In the herbivora grasses and grains are converted into living organized animal forms, including adipose tissue and muscular flesh. In them the change of grasses and grains into flesh may be considered more remarkable than in man, who lives in part upon animal food. But there is no difference. In both there has been an entire change or metamorphosis of food into blood. After the food, under whatever form it may be absorbed, enters the circulation it continues to be carried uninterruptedly to every gland and tissue of the body. It passes and repasses, so far as the glands are concerned, through the lungs, kidneys, spleen, liver, etc. Whether there is any elective affinity determining certain substances in the blood to go rather to one gland than to another—as, for instance, the kidneys and spleen—perhaps can never be ascertained. I do not think the thing so altogether improbable. Physiologists teach that so soon as the food is absorbed it goes directly to form organized tissues, muscles, bones, etc. I say that they do not sufficiently recognize the fact that the food has to be subjected to the action of every gland in the body (except those concerned in reproduction) before it is converted into blood. I teach that after the blood has been thus formed it goes to make up the tissues of the body, organized forms, from conception till full growth, and that during this period, if the individual remains in perfect health, the law of constructive assimilation has alone been at work; that the excreta thrown from the body during this period are made up of that portion of the food which was not changed into muscles, bones, etc.

We have now followed the food from the stomach to the left side of the heart. It has been clearly stated that one form of

food, the oily, is often found in the blood of the arteries and veins in all parts of the body. I believe, and I can not see how it can be controverted, that the albuminose and glucose forms of food also reach the left side of the heart and are sent along with the arterial blood to all parts of the body before they are completely changed into white blood corpuscles, lymph corpuscles, and red blood corpuscles. This brings us to consider the part performed by the lymphatics upon these three forms of food.

We copy literally from Flint's Physiology what immediately follows on the lymphatic system: "In 1649 Pequet discovered the receptaculum chyli, and demonstrated that the lacteals did not pass to the liver, but emptied the chyle into the commencement of the thoracic duct, by which it was finally conveyed into the system. In 1650-51 the anatomical history of the absorbent vessels was completed by the discovery by Rudbeck of vessels carrying a colorless fluid in the liver and finally in all parts of the body. Rudbeck demonstrated the anatomical identity of these vessels with the lacteals. They were afterward carefully studied by Bartholinus, who gave them the name of lymphatics. Though these vessels are very delicate and difficult to study, it is now pretty generally admitted that they have no orifice at their origin in the intestines and other parts, but are perfectly closed, and all substances which are absorbed by them enter by imbibition; and it is now known that the fatty portions of the food, reduced to a very fine emulsion by the pancreatic juice, are absorbed by this system of vessels, and that these are the only principles taken up in great quantity. It is an important question to determine whether the lacteals, in addition to their more important function, be not concerned in the absorption of drinks, the albuminoids, saline, and saccharine matter. Comparative analysis of the lymph and chyle always shows in the latter fluid an excess of albumen and fibrin. As we may reasonably suppose that during the intervals of digestion the lacteals carry ordinary lymph, for at this time the vessels are filled with a colorless, transparent fluid having the general physical character of lymph, it is natural to infer that the excess of albumen and fibrin in the white chyle

is due to the absorption of albuminoids from the intestinal canal. What has just been stated regarding the absorption of albuminoids applies with equal force to the saccharine matter and inorganic salts."

It is true that the products of the digestion of saccharine and amylaceous matters are taken up mainly by the blood-vessels, but a small quantity is also absorbed by the lacteals. There can be no doubt that a small portion of the liquid taken as drink finds its way into the circulation by the lacteals, though the greater part passes directly into the blood-vessels. According to the estimates of Dr. Dalton, deduced from his own observation upon dogs, and the experiments of Colin upon horses, the total quantity of lymph produced in the twenty-four hours in a man weighing one hundred and forty pounds is from six to six and one half pounds; and again, reasoning from experiments made upon dogs eighteen hours after feeding, when the fluid, passing up the thoracic duct, may be assumed to be pure unmixed lymph, the total quantity alone produced in the twenty-four hours by a man of ordinary weight would be between three and a half and four pounds (3.864 pounds). Collard de Martigny found, in comparing the quantity of fluid in the lymphatics of the neck during digestion and absorption with the quantity they contained soon after digestion was completed, that while digestion and absorption were going on actively the vessels of the neck contained scarcely any fluid, but the quantity generally increased after these processes were completed.

COMPOSITION OF LYMPH FROM A COW.

Water,	964.0
Fibrin,	0.9
Albumen,	28.0
Fatty matter,	0.4
Chloride of sodium,	5.0
Carbonate, phosphate, and sulphate of soda,	1.2
Phosphate of lime,	0.5
Total,	1000.0

The analyses of the human lymph which seem to be the most

reliable, and in which the fluid was comparatively pure and normal, are those of Gubler and Quevenne. Gubler and Quevenne made elaborate analyses of the different specimens of the fluid with the following results:

COMPOSITION OF HUMAN LYMPH.

	First Analysis.	Second Analysis.
Water,	939.87	934.77
Fibrin,	0.56	0.63
Caseous matter with earthy phosphates and traces of iron,	42.75	42.80
Fatty matter in second analysis fusible at 102.3° F.,	3.82	9.20
Hydro-alcoholic extract containing sugar and leaving after incineration chloride of sodium with the phosphate and carbonate of soda,	13.00	12.60
Total,	1000.00	1000.00

The above analyses show a much larger proportion of solid constituents than was found by Lassaigne in the lymph of the cow. It is evident, however, from a comparison of the analysis of Gubler and Quevenne, that the composition of the lymph even when it is unmixed with chyle, is subject to great variations. The caseous matter given by Gubler and Quevenne is probably equivalent to the albuminous matter of the chemists. The distinctive characters of the different principles found in the lymph do not demand extended consideration, inasmuch as most of them have already been treated of in connection with the blood. In comparing, however, the composition of the lymph and the blood, we are at once struck with the great excess of solid constituents in the latter fluid. In analyses of the serum of the blood by Becquerel and Rodier the proportion of solid constituents was ninety-two parts per thousand, while in the analyses of the human lymph by Gubler and Quevenne the proportion was from sixty to sixty-five parts per thousand; and in the analysis of Lheritier, which gives the largest proportion of solid

matter, the proportion was between seventy-five and seventy-six parts. In all analyses except those of Lheritier the organic nitrogenized compounds have been found to be very much less in the lymph than in the blood. This is generally most marked in the fibrin; but, as before stated, the proportion of all the ingredients is quite variable. Lassaigne found in the lymph of the cow that the quantity of the albumen is little less than one half the proportion contained in the blood; but in most analyses of the human lymph the proportion has been much less. The albuminoid matter of the lymph, therefore, would seem to possess certain distinctive characters, but we know so little of the functions of this fluid that it is impossible to assign to these substances any special physiological properties. Fatty matter has generally been found more abundantly in the lymph than in the blood; but their proportion is even more variable than that of the albuminoid substances. All of the constituents of the blood exist in the lymph, the only difference being in their relative proportions. It is the same with the corpuscular elements; for the so-called lymph corpuscles are identical with the leucocytes of the blood, and the red disks frequently find their way into the lymphatics. Glucose has also been found in the lymph. Poiseville and Lefort found that the proportion of sugar was always greater in the lymph than in the chyle. It has not been ascertained how the sugar contained in the lymph takes its origin, and its function in this situation is equally obscure. The presence of urea in considerable quantity in both the chyle and the lymph has been determined by Wurtz; and it is thought by Bernard that the lymph is the principal fluid, if not the only one, by which this excrementitious substance is taken up from the tissues. Though the urea always exists in the blood, its quantity is less than in the lymph. The positive facts upon which to base any precise ideas in regard to the general functions of the lymph are not very numerous. From the composition of this fluid, its mode of circulation, and the fact that it is being constantly discharged into the blood, it would seem not to have an important function in the active processes of nutrition. The experi-

ments of Collard de Martigny sustain this view, inasmuch as the quantity and the proportion of the solid constituents of the lymph were rather increased than diminished in animals that had been deprived of food for several days, while it is well known that starvation impoverishes the blood. On the other hand urea, which is one of the most important of the products of destructive metamorphosis of the tissues, is undoubtedly taken up by the lymphatics and conveyed in this fluid to the blood. It remains now for future investigation to determine whether other excrementitious principles may not be taken up from the tissues in the same way—a question of great importance in its relations to the mechanisms of excretions. What is positively known in regard to the functions of the lymph may be summed up in a very few words. A great part of its constituents is evidently derived from the blood, and the relations of these principles (fibrin, albumen, and the inorganic salts) to nutrition, are not understood. The same may be said of sugar, also a constant constituent of the lymph, the origin of which even is not known.”

Had physiologists considered that the office and functions of the lymphatics are to assist the liver, the lungs, the skin, and the kidneys in the conversion of the food into blood, they would not have been so perplexed as to the uses and purposes of the lymphatic system in the animal economy. It will be observed that Dr. Flint says that “all the constituents of the blood exist in the lymph, the only difference being in their relative proportions.” He also says that sugar and fatty emulsion are found in the lymph. We have seen that after the food is absorbed from the stomach and intestines and conveyed through the different passages already referred to, to the left side of the heart, that it, the food, has not yet become blood. It has still to be subjected to the metamorphosis of the glands, of the skin, of the kidneys, and of the spleen; but after the food in this condition has reached the left side of the heart, it is conveyed along with a volume of blood to all parts of the body and to parts in which there are no glands, viz. to the muscles, bones, etc. Now the

lymphatics originate in all parts of the body to which the arteries go, whether to glands or muscles and bones. Maintaining, as I do, that the lymphatic system is a part of the wonderful apparatus by which the food is converted into blood, I say that the lymphatics imbibe the food still floating in the blood, whether it be oily, saccharine, or albuminoid, and convey it to the lymphatic glands, wherever they may be. It is acted upon by those glands and then carried to the thoracic duct, or to the left subclavian vein, and next poured into the vena cava. That part of the food which is so taken up by the lymphatics from the blood floating in the arteries has not yet become blood, nor is it fitted to build up organized tissues.

The extracts taken from Dr. Flint's elaborate work do not follow each other consecutively. I have given, I think, sufficient to show the received doctrines of the most distinguished physiologists on the lymph and lymphatic system. The knowledge of the lymphatic system, Dr. Flint himself being judge, does not amount to much. As more than once repeated, I must again call the attention of the thoughtful reader to what I have before said, viz. that so soon as physiologists get the food into the blood-vessels they seem to ignore it altogether, and then only speak of the blood; they seem to forget that the fatty emulsion, the glucose, the albuminose, and the inorganic salts and water with which they are intimately mixed may float in the blood for an undetermined period, until they themselves are converted into blood by glandular action. This has been the fruitful source of the unintelligibility of the lymphatic system, and of the lymph, its product. Except the uses of the lacteals, which take up the fatty constituents of the food from the intestines by imbibition, they really know nothing about it. Sugar and fatty matter have been found in the lymph as well as in the blood. Physiologists can not account for it. They can not tell how these two substances get into the lymph, and what they have to do there. Albuminose, not being so easily detected, they say nothing more about it after it gets into the portal vein, but after that only speak of albumen. I say that oily and saccharine matters float-

ing in the arteries are taken up by the lymphatics. They also, no doubt, act upon the nitrogenous form of food which has not been completely metamorphosed when it first reaches the left side of the heart. As a demonstration of the correctness of my views, I call the attention of the reader to the observation of Collard de Martigny. He found, in comparing the quantity of fluid in the lymphatics of the neck during digestion and absorption with the quantity they contained soon after digestion was completed, that while digestion and absorption were going on actively the vessels of the neck contained scarcely any fluid, but the quantity generally increased after these processes were completed. It will be seen that while digestion, in the sense here referred to, is going on, and indeed up to the time that all the food has been absorbed and conveyed through the lacteals, through the liver, and through the lungs to the left side of the heart, the lymphatics remained inactive. The time for them to begin to work was after the three forms of food, not yet entirely changed, had been sent along with the arterial blood to all parts of the body. What M. de Martigny observed in the lymphatics of the neck applies, no doubt, to lymphatics in every other part of the body. Again, Poisenille and Lefort found that the proportion of sugar was always greater in the lymph than in the chyle. As the lacteals take up the oily and not the saccharine form of food, and as the three forms of food floating in the blood do not pass at all through the lacteals, it can be readily seen how more sugar is to be found in the lymph proper than in the chyle.

Dr. Flint, vol. 3, page 16, says, "Certain of the fluids are formed by special organs, and have important functions to perform which do not involve their discharge from the organism. These may be classed as the true secretions, and the most striking examples of them are the digestive fluids." And again, in vol. 2, page 430, says, "In the general description of the lymphatic system, three sets of vessels are usually recognized—the plexus, situated on the general surface; the deep vessels; and those coming from the small intestines, ordinarily called lacteals." Lymphatic vessels are found in the largest number in organs

to which large quantities of blood are sent in proportion to the size of those organs or glands. They are compared by physiologists to other secreting glandular organs; as, for instance, to the glands distributed over the mucous surface of the small intestines.

I believe that all who will consider their functions must agree with me that they are secreting organs. That portion of the lymphatic system known as the lacteals absorbs the chyle from the intestinal canal. All other portions of the lymphatic system, those distributed to the deep vessels and the plexus on the surface, imbibe from the arteries which they accompany the thing we call lymph. As lymph contains every constituent of the blood, and as the blood is undeniably formed from the food, how can we avoid the conclusion that the lymph too is formed from the food. It is simply one of the stages through which the food passes in its transformation into blood. Dr. Flint, and all physiologists, I believe, teaches that "the so-called lymph corpuscles are identical with the leucocytes, or white blood corpuscles, and the red disks frequently find their way into the lymphatics." I agree with those physiologists who maintain that the lymph corpuscles are the first forms of the red disks or blood corpuscles. Dr. Flint states that "the proportion of the solid constituents of the lymph were rather increased than diminished in animals that had been deprived of food for several days." I do not think that physiological laws are to be explained by pathological conditions; but in the case referred to I think there may be an explanation in this—the lymphatics following the arteries to every and all parts of the body, exhaust from them the last elements of food contained in the blood after the animal begins to suffer from the want of its usual supply of food. As to the existence of urea in the lymph, or in the blood of a man in perfect health, I deny the fact, and I do not doubt that physiologists will come to agree with me when they have made further examinations of both of those fluids. But I can not consider that question in this connection.

The lymph is a secretion proper; but, unlike other secretions, it is formed not from the blood but from the three forms of food floating in the blood which had not been changed when they reached the left side of the heart. All other secretions, such as the saliva, the gastric, and pancreatic juices, are formed directly from arterial blood. I consider the liver, the lungs, the skin, and the kidneys as excretory organs; what they separate from the food in transforming it into blood has to be thrown off from the body.

The excretions will be considered in the next chapter.

In his concluding notices of the lymph, Dr. Flint says, "From the composition of this fluid, its mode of circulation, and the fact that it is being constantly discharged into the blood, it would seem not to have an important function in the active processes of nutrition." If the food becomes blood as soon as it enters the circulation by some mystic catalytic process, or if, without being turned into blood, it is transformed into living organized tissues by simply passing through the capillary system, as seems to be taught by Flint and Dalton, the two great American physiologists, I could agree with them that the lymph "had no important function in the active processes of nutrition." But if the views which I have presented above be correct, then it must be apparent to every reflecting mind that the lymph performs a very important part in the processes of nutrition. The quantity of the lymph is admitted to be large; this large quantity is poured into the vena cava just before that great vessel empties itself into the right side of the heart.

What is immediately about to take place? The venous blood will immediately lose a large part of its constituents—carbonic acid—in its passage through the lungs. It also loses a large amount of watery vapor, carrying off with it perhaps some inorganic salts. The lymph, rich in albumen, fibrin, and lymph corpuscles, comes to supply this loss. The large amount of fluid poured into the blood from the great lymphatic vessels may supply the loss in the lungs thrown off as watery vapor. I do not

here consider the offices of the lymphatics when the human body is in a pathological state.

There is another organ which acts upon the food after it reaches the left side of the heart, which organ has no excretory duct. I refer to the spleen. Many volumes have been written upon the functions of this gland. That organ, for the size of it, is supplied with a very large quantity of arterial blood. The albumen and fibrin found in the blood of the veins coming from the spleen are much larger in amount than the quantity of those elements found in the arterial blood entering that gland. Dr. Flint says, "Both Beclard and Gray observed a marked increase in the fibrin and albumen in the splenic vein." I do not think that this fact is contradicted by any physiologist. As I can not believe that the catalytic change which converts food into blood takes place in the blood-vessels, I maintain that one of the functions of the spleen is to convert the albuminose of the food into fibrin and albumen. From what other source can this increased quantity of albumen and fibrin have come than from the food in the form of albuminose contained in the blood passing through the spleen? The spleen is continually pouring rejuvenated blood into the portal vein. It is because physiologists have not considered that glueose, fatty emulsion, and albuminose are not blood, and do not become blood in the circulatory system, but have to be subjected to the action of secretory glandular organs to convert them altogether into blood, that they have been unable to understand the physiological uses and offices of the spleen. Physiologists have seemed to marvel because the spleen is a ductless gland. If my explanation of its functions is correct, no duct is needed. Nothing is separated from the blood in its passage through the spleen. The work which the spleen has to do is to change certain elements of the food into albumen and fibrin. But it will be said that animals not only live but thrive after the spleen is taken out of their bodies. I doubt that they thrive much. This only shows the wonderful provision which Nature has made for such accidents, whether caused by sickness or the vivisection of experimenters. In some fever and ague countries

the spleen sometimes becomes so much enlarged that it is unable to perform any physiological function. In such cases, or where the spleen is removed, other organs convert the food into albumen and fibrin. In perfect health, other glands contribute to that end.

The portal veins have no valves. The lymphatics have valves.

CHAPTER V.

THE EXCRETIONS.

We copy from Flint's Physiology: "Certain other of the fluids are composed of water holding in solution one or more characteristic principles which result from the physiological waste of the tissues. The principles have no functions to perform in the animal economy, and are simply separated from the blood to be discharged from the body. These may be classed as excretions, the urine being the type of fluids of this class." (Vol. 3, p. 16.)

"All the remarks, indeed, that have been made concerning excretions in general may be applied without reserve to the action of the kidneys; and there are few subjects in physiology of greater interest than the process of urinary secretion, with its relations to nutrition and disassimilation." (Vol. 3, page 144.)

"The striking peculiarities which the kidney presents in its structure, as compared with the true glands, and the fact of the voluntary discharge of its secretion at certain intervals, would naturally lead to a close study of the mechanism of the formation of the excretions. The composition of the urine also will be found exceedingly complex, and its various ingredients bear the closest relation to the process of nutrition and disassimilation, all of which considerations render it of the greatest importance to ascertain the precise mode of its formation, and to study all the conditions by which this process may be modified. In the present state of our knowledge, we must certainly regard the excrementitious constituents of the urine as formed essentially in the system at large and merely separated from the blood by the kidneys, and a consideration of these effete principles belongs to

the subject of nutrition. It remains for us, then, in this connection to treat in general terms of the way in which these substances find their way into the urine. The most important constituent of the urine is urea, a crystallizable, nitrogenized substance which is discharged by the skin as well as by the kidneys. This has long been recognized as an excrementitious principle, but the first observation which gave any definite idea of the mechanism of its production was presented by Prevost and Dumas in 1821. Since that time, as the processes for the determination of urea in the animal fluids have been improved, this substance has been detected in minute quantity in the normal blood by Marchand, Picard, Porsenille, Goble, and many others. Picard, indeed, carefully estimated and compared the proportions of urea in the renal artery and the renal vein, and found that the quantity in the blood was diminished about one half in its passage through the kidneys. According to Robin, who apparently accepts the results obtained by Picard, the blood in the renal vein contains much less urea, urates, creatin, creatinin, chloride of sodium, etc. than the blood of the renal artery. Still later urea has been found by Wurtz to exist in the lymph and chyle in larger quantity than even in the blood.

“These facts, which have been almost universally regarded as established, have led physiologists to adopt the view that the peculiar excrementitious principles found in the urine are not produced by the kidneys, but are formed in the system by the general process of disassimilation, are taken up from the tissues by the blood, either directly or through the lymph, and are merely separated from the blood in the kidneys; and it has consequently been pretty generally assumed that nearly, if not all, the constituents of the urine preëxist in the circulatory fluid. There is indeed no well-defined principle in the urine that has not been demonstrated in the blood. As an accidental argument in favor of this view of the mechanism of urinary excretion, it has been ascertained that when the kidneys have been interrupted in their function there is a tendency to the elimination of the excrementitious principles of the urine by the lungs, skin,

and alimentary canal, and that these matters only accumulate in the blood after this vicarious effort has failed to effect their complete discharge." (Flint, vol. 3, page 165.)

In Flint's Physiology, vol. 2, page 528, he says, "One of the most important physiological facts in the chemical history of the lymph is the constant existence of a considerable portion of urea. This can not be derived from the blood, for its proportion is greater in the lymph, notwithstanding that this fluid is being constantly discharged into the blood-vessels. The urea which exists in the lymph is derived from the tissues; it is discharged then into the blood, and is constantly being removed from this fluid by the kidneys." That urea, the nitrogenized principle found in the largest quantity in the urine, is an excrementitious substance none will deny, but that the urea which exists in the lymph is derived from the tissues, I deny. It ought to be a sufficient answer to this assumption, for it is the merest assumption, growing out of a total ignorance of the laws of the animal economy; it ought, I repeat, to be sufficient to state the fact that urea does not exist in any living tissue found in the human body when the individual is in perfect health. Effete matter, as such, results from the disintegration, disassimilation, or destructive metamorphosis, call it which you please, of living, organized tissue. The process is accomplished by the capillary vessels. Arterial blood is carried to them in every part of the body. The veins do not make, but they receive, a different fluid conveyed into them by the capillaries. This different fluid is the blood changed by its passage through these last-named vessels. It is changed or rendered darker in color by the addition of effete matter eliminated in the capillaries. It may be effete blood. In health, when the individual is grown and remains about the same average weight, there is little or no addition to or subtraction from the organized living parts of the body. In sickness, where a man is decreasing in weight daily, the process of disassimilation is actively going on, and the venous blood is charged with a larger amount of effete matter. But, in whatever way it may be separated from the living organism, it is always

made up of constituent parts of that organism. Urea, while an excrementitious principle, is also a secretion. Dr. Flint says it can not be derived from the blood because it exists in the lymph in larger quantities; and again, that the urea which exists in the lymph is derived from the tissues. I wish the reader to note well this last expression. Dr. Flint does not say, nor does any physiologist say, how it is derived from the tissues. I have quoted sufficiently to show the doctrines taught on the subject by the highest authorities. I fully agree with Dr. Flint in the importance he attaches to a correct understanding of the excretions and of the method by which the urine is formed, which fluid, he says, is a type of them. Dr. Flint says that urea has been detected in minute quantities in the normal blood by many distinguished physiologists. By normal blood I suppose he means the pure arterial blood of a healthy man. If, as he teaches, and all physiologists to whom he refers teach, urea is the product of destructive matamorphosis, and is created or formed by some process in the act of separation of the solid constituents of the organism, then it not only ought to be found in minute quantities but it ought also to be found in large quantities in the normal blood. If it is ever a constituent part of the normal blood of a healthy man it is a constant quantity, and must always be found there. We will see presently whether it is a constant quantity in healthy blood.

The doctrine taught by physiologists involves the necessity for and the truth of another doctrine, viz. that constructive assimilation and destructive metamorphosis are continually going on in the animal body at the same time. If a man consumes three or ten pounds of food and drinks daily, that quantity is added to the solid organism, and the like amount is separated from the organism by destructive metamorphosis, and they correctly call the thing so separated effete matter. Urea, they say, is a constituent part of effete matter. Dr. Dalton says, "The quantity of urea produced and discharged daily by a healthy adult is, according to the experiments of Lehman, about five hundred grains. It varies to some extent like all the other

secreted and excreted products with the size and development of the body. Lehman, in experiments on his own body, found the average daily quantity to be four hundred and eighty-seven grains. Professor William A. Hammond, who is a very large man, by similar experiments, found it to be six hundred and seventy grains. Dr. John C. Draper found it to be four hundred and eight grains. The three individuals named were, I take it, in good health, as nothing is said to the contrary. These quantities of urea, relative in proportion to the size of the body, must be emptied into the blood of every healthy man during every twenty-four hours, according to the doctrine taught by physiologists. It is, according to them, one of the forms of effete matter. This effete matter is being constantly separated from the solid organism of healthy individuals, according to their teaching. It is then, I again affirm, a constant quantity of the normal blood.

I do not see how physiologists can avoid this conclusion. How, I would like to know, do they explain what they assume to be a fact, that urea is found in minute quantities in the normal blood? It exists, necessarily according to their teaching, in very large quantities. Why then do they not always find it in large quantities in the blood? This is no idle question. Physiologists are compelled to answer it or acknowledge that they are altogether mistaken as to the origin of urea. According to the three observations just given, there were discharged from the bladder, in one instance, five hundred grains of urea; in another, six hundred and seventy grains; and the last, four hundred and eight grains. Now, according to the doctrine taught by physiologists, these several large quantities of urea existed in the blood of healthy men; it was a part of effete matter separated from the tissues, the functions of the kidneys being only to separate the urea from the blood. M. Claude Bernard and some other experimenters admit that they could not find urea in the blood of dogs from whom the kidneys had been removed, sometimes for twenty-four hours and sometimes for forty-eight hours after the removal of those organs. Destructive metamorphosis

was no doubt certainly going on in the entire organism of those dogs so mutilated, and a large amount of effete matter must have been emptied into the blood. Urea, they say, is not only a constituent part of effete matter, but a very large part of it. Why then, I ask, did they not find it in the blood of those dogs? The kidneys, they say, separate urea from arterial blood brought to them by the renal arteries. These glands do not, according to their theory, generate or create urea from the blood, but only separate it as a compound entity which preëxisted in the blood before that fluid entered the kidneys. According to their teaching the quantity of urea in the blood must have begun to accumulate immediately after the kidneys were taken out of the bodies of the dogs, for they were the glands, the especial function of which was to eliminate urea from the blood. Not only common sense but inexorable logic compels them to explain the dilemma in which they find themselves or to admit that they are entirely in error.

I will give another illustration to show that after the administration of a particular drug no urea can be found in the blood. I refer to colchicum. In the Monthly Journal of Medical Sciences for January, 1852, page 21, in a paper by Dr. J. M. MacLagan, of Edinburgh, on the Effects of Colchicum, we find the following: "The first case was that of a girl under Dr. Wright's care in the Royal Infirmary. On the 13th of October a small quantity of blood was abstracted, analyzed, and found to contain in

"1000 parts of blood,	. . .	0.507 urea.
1000 " " "864 uric acid.

"The urine was examined at the same time. It contained:

"Total solids,	. . .	28.568
Water,	. . .	971.432
Urea,	. . .	10.496
Uric acid,257
Inorganic salts,	. . .	7.461
Organic matter,	. . .	10.354
Total,	. . .	1000.000
		28.568

"Colchicum in combination with muriate of morphia was then administered. The urine was again examined on the 18th of October, being the fifth day. It contained:

"Total solids,	.	.	.	31.459
Water,	.	.	.	968.541
Urea,	.	.	.	12.312
Uric acid,421
Inorganic salts,	.	.	.	8.231
Organic matter,	.	.	.	10.495
Total,	.	.	.	1000.000 31.459

"The urine was again examined on the 22d of October, or ninth day. It contained:

"Total solids,	.	.	.	35.613
Water,	.	.	.	964.387
Urea,	.	.	.	13.984
Uric acid,598
Inorganic salts,	.	.	.	9.401
Organic matter,	.	.	.	11.630
Total,	.	.	.	1000.000 35.613

"After twelve days' constant use of the colchicum, a small quantity of blood was procured for examination. Now, however, not the slightest trace of urea, or uric acid, could be detected in so large a quantity as thirty-five hundred grains. The urine was examined at the same time and found to contain:

"Total solids,	.	.	.	34.554
Water,	.	.	.	965.446
Urea,	.	.	.	14.561
Uric acid,737
Inorganic salts,	.	.	.	9.649
Organic matter,	.	.	.	9.607
Total,	.	.	.	1000.000 34.554

"The colchicum being continued the urine contained on the eighteenth day:

"Water,	.	.	.	961.872
Urea,	.	.	.	17.635
Uric acid,	.	.	.	1.034
Inorganic salts,	.	.	.	9.049
Organic matter,	.	.	.	9.607
Total,	.	.	.	1000.000

Dr. Maclagan does not give another case in which the blood was examined after the administration of colchicum. I have no doubt if I had access to recent medical books and journals, and could examine them myself, I should find many cases confirmatory of the truth of Dr. Maclagan's observations. I would call attention to the fact observed by Dr. Maclagan, that the quantity of urea in the urine was greatly increased after the administration of colchicum. In the case referred to 10.496 parts of urea were found in one thousand parts of urine before the administration of the drug. When the blood was examined the second time the quantity of urea in the urine was increased to 14.561 parts in one thousand parts of urine. And afterward the urea was increased to over seventeen parts per thousand of urine in this same patient. Now it will be observed in this case that before the administration of colchicum half a grain of urea was found in one thousand grains of blood; after the administration of the drug not a trace of urea could be found in three thousand five hundred parts of blood. If urea is a constituent part of effete matter, if it is formed in some manner in the act of its separation from the living organism, it must necessarily be a constant quantity in the blood; and seeing that under certain circumstances or conditions, to be more particularly referred to hereafter, the quantity of urea found in the urine is largely increased, we must have under those conditions a corresponding increased quantity of it in the blood. The case referred to by Dr. Maclagan was the subject of rheumatism, and was not in good health. So far as the secretions and the excretions are concerned, I do not know that the laws of organic or vegetative life in the higher orders of animals, as the ox, for instance, differ from the same laws in the human animal, man. If urea is formed from and by disintegration of living tissues, in the act of waste and repair in the body of a man, it ought to be so formed in the body of an ox. If I am not correct in this I would like to be informed wherein the error consists.

Now we know that so long as a calf sucks a cow urea is found in the urine; but so soon as the calf is weaned and

feeds on grasses and grains, the urate of soda ceases to appear in the urine, and in its place we find the hippurate of soda. According to the theory of physiologists as to the formation of urea, this can not be explained. The effete matter formed from the muscles, nerves, brain-substance, and bones of an ox can not differ from the effete matter formed from the same organized forms in a human body. There may be more or less of any one constituent part of this effete matter, but as a whole it must contain the same chemical substances. The human infant, in one respect, is like the calf; so long as the latter is nourished on animal food, milk, urea and urate of soda are found in its urine. In new-born infants, and for a short time after birth, no urea is found in their urine; but in a very short time it appears in that fluid. What has happened during this short interval? Simply this, the child has begun to suck, and it has to convert the mother's milk into blood; urea is one of the products of this conversion, just as it was one of the products of the conversion of milk into blood in the calf. But why is there no urea discharged from the bladder of a new-born infant, or up to the time the child sucks? No physiologist will pretend to say that the laws of secretion and excretion in an infant a month old differ from the laws which govern the vegetative life of an infant at birth. The laws of constructive assimilation and destructive metamorphosis which, they say, are continually and coexistingly at work, are then in active operation, and according to their theory, a large amount of effete matter containing urea must be constantly eliminated from the body.

I have shown that under two conditions urea can not be found in the blood. I have shown that under one condition it is found in the urine of the herbivora, and that under another condition it not only can not be found, but that it does not exist in the urine; another substance, hippurate of soda, taking its place. I have also shown that before a human infant sucks, urea can not be found in its urine. I think it is not an assumption in me to say that urea is not a

constituent part of healthy blood, and to affirm that it was never found in the blood of a healthy man whose kidneys were performing their proper functions. If I am correct in this, it necessarily follows that urea is not formed from, or out of, effete matter. What applies to urea applies to all other organic forms found in the urine. The kidneys, then, do not remove effete matter under any form from the blood. This applies to men and women in good health or in ordinary health. When the body is being wasted by sickness or starvation, a pathological state, when no food is taken for days, then the kidneys may form urea and the other organic substances found in the urine from effete matter. But this, I repeat, is a law of life under pathological, not physiological, conditions. How then are the urea and the other organic constituents found in the urine formed? I answer that they are one and all of them formed from the food. The urine contains the same four classes of substances as does the food, viz. water, nitrogenized substances, non-nitrogenized substances, and ashes. It follows, then, that it *can* be formed out of the food. Dr. Dalton says, "The quantity of urea varies also with the nature of the food." Lehman, by experiments on his own person, found that the quantity was larger when living exclusively on animal food than when living on a mixed or vegetable diet; and that the quantity was smallest when confined to a diet of purely non-nitrogenous substances, as starch, sugar, and oil.

The following table gives the results of these experiments:

Kind of Food.	Daily Quantity of Urea.
Animal food,	728 grains.
Mixed food,	487 "
Vegetable food,	337 "
Non-nitrogenous food,	231 "

It is not stated that in these experiments a less quantity of mixed or purely vegetable food was eaten when so much less urea was found in the urine, than of animal food when so large a quantity was found. The reasonable supposition is that the individuals ate enough to nourish and sustain their bodies, and therefore they consumed more of mixed and vegetable food than

they did of animal food. But physiologists teach that all the food eaten, whether animal or vegetable, goes to form living tissue, muscle, bone, nerve-substance, etc. before any of it is eliminated from the body by the excretory organs. According to them, the urica which is found in the urine was once a part of the living organism. We know that every part of the animal body can be formed from vegetable substances. This is constantly exhibited in the bodies of the herbivora, and millions of human beings who have well developed bodies live almost exclusively on vegetable food. I have already shown that it can not be formed by this disintegrating process, because it does not exist in the urine of an ox or of a new-born infant, and that it is only under particular circumstances, pathological conditions, in which it can be detected in the human blood. Every reading man is familiar with the dietetic habits of the Esquimaux. According to Dr. Kane they eat from eight to ten pounds of uncooked meat at a meal, and drink half a gallon of soup and water daily. He saw one of these men eat at a meal ten pounds of fresh meat. Can any one suppose that this enormous quantity of food and drink is daily converted into organized living tissue, and that it is all removed within the same short period of time from the body as effete matter, and that urea and other nitrogenized principles in corresponding quantities are produced in that way? The thing is absurd. The Esquimaux are said to be men of moderate stature. If they weigh only one hundred and forty pounds, they would, according to an estimate of Dr. Dalton, given in a former part of these pages, have their whole body made up of new walrus blubber every fourteen days. It seems to me that animals or men so continually re-formed, and that in so short a time, could have no strength; and yet the Esquimaux are said to be remarkably strong and active. They require a large amount of food to keep their bodies warm in so cold a climate. Animal heat in them is produced by the activity of secretory and excretory processes, by which this large amount of food and drink is converted into blood. No doubt they discharge daily large amounts of urine, and this fluid contains, no

doubt, very large quantities of urea and other organic constituents. I maintain that the kidneys are the organs, and the only organs, which make or create urea and all other organic substances found in the urine. We have seen that under the influence of colchicum they are stimulated to form large quantities of urea. Dr. Dalton says, "The daily quantity of urea varies also with the mental and bodily activity." It is a well-known fact that certain mental conditions cause the kidneys to secrete urine in large quantities. The action of that class of medicines known as diuretics is another example of this. It all amounts to and shows one thing, viz. that the kidneys, like the liver, can be excited to increased or more vigorous action. It would be impossible to understand why physiologists are unwilling to admit that the kidneys form or create all organic substances found in the urine did we not know that the truth or soundness of another theory, which is made to depend upon the assumption that the kidneys simply separate effete matter under different forms from the blood, rests upon the denial of this vital law. Urea is found as a constant quantity of urine; under certain dietetic and therapeutical conditions, this quantity is largely increased. Urea is not found in the blood under certain conditions to which I have referred. But urea is sometimes found in the blood in very minute quantities. Physiologists have assumed that all effete matter is thrown out of the system through three channels—the lungs, skin, and kidneys. Except the cholesterin, they teach that the bile is an incrementitious substance, and is constantly being reabsorbed. The lungs throw off carbonic acid with watery vapor, with perhaps some other substances under particular conditions not necessary to be named here. The skin throws off from the body a very small fraction of solid matter, being about five parts in a thousand. It remains then as a necessity, according to their theory, for the kidneys to remove from the blood a very large amount of the effete matter, which is thrown off from the body daily. Physiologists having also assumed that all the food, after its absorption from the stomach and bowels, is transformed into living tissue before any of it is removed as effete matter, they are shut up to

the necessity of affirming that nearly all of the effete matter is removed from the body by and through the kidneys. This last assumption is made to depend upon another, viz. that the body of a man weighing one hundred and forty pounds is entirely re-formed in twenty days according to some, and in a few months according to others. Urea having sometimes been found in the blood of a man in ordinary health, and also in the neighborhood of certain tissues, they argue that it is formed or created somewhere or somehow in different parts of the body, and that the only office of the kidneys is to remove it and other compound substances from the blood.

As I will discuss this subject again under another head, I will only here repeat. I affirm, first, that the kidneys by glandular and catalytic action form urine and all its constituent parts from the food floating in the blood; that this is one of the processes by which the food and drinks are converted into blood preparatory to their assimilation into the organized forms of the animal economy; that the compound substances, such as urea, creatin, creatinin, etc. found in the urine are the necessary products of the vital action which prepares the food for the transformation into muscle, bone, nerve, and brain-substance, and all of the other organized forms of the animal body. I maintain, secondly, that the kidneys do not act upon and never separate effete matter from the blood of a man while he is in good health; and, as a consequence of this last proposition, that urea, creatin, creatinin, etc. were never parts of the living organism. This is the condition of things in health, and it expresses a physiological action of the kidneys when constructive assimilation alone is going on in the animal body. That the kidneys may act upon effete matter when constructive assimilation is not at work, but when from starvation or sickness no food is eaten, and destructive metamorphosis is actively at work, causing a loss of weight. I admit that the kidneys then act upon the effete matter in the blood as they do upon medicinal substances, such as the iodide and bromide of potash, etc. I must now refer to this subject under another head.

We are told that the urea exists in the lymph and chyle in larger quantities than in the blood. In the extracts found in the first part of this chapter Dr. Flint says, "These facts which have led physiologists to adopt the view that the peculiar excrementitious principles found in the urine are not produced by the kidneys, but are formed in the system by the general process of disassimilation, and are taken up from the tissues by the blood either directly or through the lymph." Again, Dr. Flint says, "One of the most important physiological facts in the chemical history of the lymph is a constant existence of a considerable portion of urea. This can not be derived from the blood, for its proportion is greater in the lymph, notwithstanding that this fluid is being constantly discharged into the blood-vessels. The urea which exists in the lymph is derived from the tissues; it is discharged then into the blood, and is constantly being removed from this fluid by the kidneys." That urea is a constant quantity of the urine, there can be no question. If it constantly exists in the lymph, as Dr. Flint affirms, it must always be found in the blood, unless it is eliminated from that fluid by the lungs, which he does not teach or maintain. I have shown conclusively that under several conditions urea can not be found in the blood. I leave it to the physiologists to explain the dilemma in which they find themselves.

But they teach that urea is also found in the chyle. At what particular point in the chyliferous system they obtained the chyle in which they found the urea, they do not state. If between the mucous membrane of the intestinal canal and the mesenteric glands, I would like for them to explain how the effete matter got there, unless lymphatic vessels accompany the lacteals and empty themselves into them, which nobody teaches. I very much doubt the fact of urea being found in the chyle, but if my doubt is not well founded, then this particular urea must somehow be formed from the food, and not by any process of disassimilation. Dr. Flint says, "Urea is taken from the tissues of the blood either directly or through the lymph." My explanation of the existence of urea in either the blood or the lymph is

this: In certain pathological conditions, by some obstruction of the uriniferous ducts which convey the urine after it is secreted to the ureters, it sometimes happens that all the urica and other compound solid substances are not emptied into those canals, but are taken up by the venous absorbents and are emptied into the renal veins; just as in jaundice after the bile is secreted in the liver it is not all conveyed into the hepatic duct, but is partly absorbed by the veins and carried along through the hepatic vein to the vena cava. That more urica should be found in the blood of the renal artery than in the blood of the renal vein, I do not think it difficult to explain, nor do I think it more difficult to explain why more urea is found in the lymph than in the blood. The urea which has escaped through the renal veins enters the general circulation, arterial as well as venous. The lymphatics, according to my belief, accompany the arteries to separate from them, as their special physiological function, that portion of the food which has entered the arterial blood; this they convert into albumen and fibrin. But they may also possess the power to separate from the arterial blood any foreign or abnormal substance which may have gotten into it. Both of these functions, I think, are performed in health before the arterial blood has entered the capillaries. But another difficulty occurs. Agreeing fully with physiologists that more urica is found in the lymph than in the blood, we know that the whole volume of the lymph is being continually emptied into the right side of the heart, and that therefore the urica found in it must be again mixed with the general current of arterial blood. How then can we explain the fact of a larger quantity of urica being found in the renal artery than in the renal vein? I say that it escapes from the renal vein in very small quantities. After a while the lymphatics have separated it from the arterial blood and emptied it into the right side of the heart through the larger lymphatic vessels; then the law of elective affinity, to which I have once before referred, causes the urica to go directly to the kidneys through the renal arteries. We know that some forms of potash, the iodide, perhaps the bromide, are found in the urine within twenty min-

utes after they are swallowed. When the bromide of potash is taken in twenty- or thirty-grain doses, one half of it is eliminated from the blood and passes off with the urine in twenty-four hours. Urea has been given as a medicine. Let it get into the blood as it may, it is a foreign substance in the same sense that the iodide and bromide of potash are foreign substances. If by elective affinity or elective attraction the one goes directly to the kidneys, I see no reason why the same cause may not determine the other to go there. But there is perhaps another explanation. The kidneys, like all other glands which secrete actively, are abundantly supplied with lymphatic vessels. Seeing that they do take up urea in other parts of the body and discharge it into the larger lymphatic vessels, explain it as you may, it is only reasonable to suppose that they perform the same office in the kidneys when the ureters fail to receive all the urea and other compound substances which had been secreted or formed by the glandular action of those organs. There would be, in the case supposed, a much larger amount of urea, turned from its natural channel, found in the kidneys than in any other part of the body. I say that the lymphatics distributed through the kidneys would take it up, and that they would soon empty it into the thoracic duct. Urea thus conveyed into the lymph would be immediately mixed with the chyle as it is poured into the receptaculum chyli. Urea so conveyed from the kidneys into the circulation would cause a larger amount of it to be found in the blood of the renal artery than is found in the blood of the renal vein. But let it be understood that I deny that urea is a constituent part of or a constant quantity in either the blood or lymph of a healthy man. But urea is found in the blood some twenty-four or forty-eight hours after the kidneys have been removed from the body of a dog; and in the dogs experimented on it could not be found until after the expiration of twenty-four or forty-eight hours. This urea, I admit, was not formed in any manner by the kidneys. It could not be, for there were no kidneys to make it. How then was it formed? Recollect that it was not found in the blood in the dogs experimented on for many hours after

their mutilation. According to the theory which physiologists so strenuously maintain, it must always exist in the blood; indeed they say in so many words that they had detected it in normal or healthy blood. They say also that the kidneys constantly remove it from the blood. How then can they account for its not accumulating in the blood after the kidneys had been removed? I think it will puzzle them to answer this question. I say that when the organs whose function it was to form urea out of the blood had been removed from the body, some other gland had to act vicariously for the kidneys. Physiologists say that urea, under certain pathological conditions, is separated from the blood and thrown out of the body by the skin, the lungs, and the mucous surface of the alimentary canal; but they do not say that it is formed by the skin, lungs, or mucous membrane. It is a thing which I will not pretend to explain. I will only venture to conjecture that as the lymphatics have the power to separate it from the blood, they may have the power, that is, the lymphatic glands including the mesenteric glands, to form it out of materials floating in the blood.

The kidneys are, in my opinion, as much concerned in sanguification or the conversion of the food into blood as are any other glands. We have seen that our food and drinks are largely intermixed with the saliva and other juices. The kidneys remove a large portion of this watery intermixture. They also take up certain solid constituents of the food, which they convert into urea and other substances found in the urine. This explains why the blood flowing from them through the renal veins is purer than that brought to them by the renal arteries. It is perhaps the purest blood in an animal body because it has been subjected to the last elimination. In conditions of perfect health, when the blood formed from the food is not converted into tissues, then the kidneys, as well as the lymphatic glands, act upon effete blood passing through them. In pathological conditions, where effete matter is separated from the tissues, the kidneys and the lymphatic glands act upon this effete matter and convert it into urea and other solid substances.

I will now consider the perspiration.

"In the fullest acceptation of the term, perspiration embraces the entire function of the skin as an excreting organ and includes the exhalation of carbonic acid, as well as watery vapor and organic matter. The office of the skin as an eliminator is very important, but the quantity of excrementitious matter, with the properties of which we are well acquainted, such as carbonic acid and urea, thrown off from the general surface, is small as compared with the amount exhaled by the lungs and kidneys. One of the well-known objects of cutaneous exhalation is to keep down animal temperature by evaporation where there is a tendency to too great development of heat by exercise or from other causes. Physiological chemists have detected urea and some other effete matters in the perspiration, but it is probable that some volatile principles are eliminated by the general surface which have thus far escaped observation." (Flint's Physiology, pages 131-133.)

Here, again, we find that Dr. Flint classes urea and other substances found in the perspiration as effete matter. By referring back the reader will see how large an amount of saliva, gastric, pancreatic, and intestinal juices is mixed with the food in addition to the water and other fluids we drink. He will also notice the chemical constituents of the saliva, gastric, and intestinal juices. All of these are intimately mixed with the food.

Dr. Dalton, in his Physiology, page 307, says, "The daily quantity of all the fluids thus secreted and reabsorbed during twenty-four hours will enable us to illustrate the activity with which endosmosis and exosmosis go on in the living body. In the following table the quantities are all calculated for a man weighing one hundred and forty pounds.

"SECRETED AND REABSORBED DURING TWENTY-FOUR HOURS.

"Saliva,	20.164	grains or	2.880	pounds.
Gastric juice,	98.000	"	14.000	"
Bile,	16.940	"	2.420	"
Pancreatic juice,	13.104	"	1.872	"
Lymph,	27.048	"	3.864	"
	175.258	"	25.036	"

“A little over twenty-five pounds, therefore, of the animal fluids transude through the internal membranes and are restored to the blood by reabsorption in the course of a single day. It is by this process that the natural constitution of the parts, though constantly changing, is still maintained in its normal condition by the movement of the circulating fluids and the incessant renovation of their nutritious materials.”

All of these fluids are concerned, and that is their sole purpose, in the conversion of our food and drinks into blood. It is a wonderful work they have to perform. By an elective affinity, bone, muscle, membrane, tissues of all sorts, nerve, and brain-substance select from the blood that which is fitted to give them strength and nourishment. But in thus preparing the food for its wonderful uses, certain substances have to be removed from it. This, in my opinion, is accomplished before the question of effete matter can begin to be considered. That which is removed from the body is called excretory matter; but as before remarked this excretory matter is removed by secretory action. I think this applies as well to the skin as to the kidneys. In both instances arterial blood is sent to those organs, viz. to the kidneys and to the perspiratory glands.

COMPOSITION OF THE PERSPIRATION.

Water,	995.00
Animal matters and lime,	.10
Sulphates and substances soluble in water,	1.05
Chlorides of sodium and potassium and spirit extract,	2.40
Acetic acid, acetates, lactates, and alco- holic extracts,	1.45
Total,	<u>1000.00</u>

It will be seen that nine hundred and ninety-five in a thousand is water in the perspiration. According to Dr. Dalton a man of average size in good health loses about two pounds per day by perspiratory action. But the quantity varies according to the temperature, dryness, or moisture of the atmosphere, according to the amount of exercise and labor, and also according to

the quantity of fluids drunk. Indeed there is perhaps no one of the excretory functions more variable in different individuals than that of the perspiratory action of the skin. I am speaking of the question physiologically. As such, I say that perspiration is a secretion, though it removes matter which is properly called excretion. But there are some forms of disease in which it can not be called a secretion. The night-sweats of consumptive patients, and in the profuse sweats during the collapse of cholera, I think the process has ceased to be secretory and may be called effusion, or simple pouring out of the watery parts of the blood from the body. But the true physiological perspiration of a healthy man, I repeat again, is secretory action, though performing an excretory function, the purpose, object, or intent of which is to remove from the food a large part of the fluids with which it has been mixed up in its transformation into blood. In health effete matter never enters it. Has any physiologist ever even alluded to the fact that perspiration is one of the forces or functions by which the food is converted into blood; that it is one of the methods by which the fluids which have been mixed up with the food are removed from the body? Of course under the general term of food I include water and other fluids drunk as well as solid substances eaten. We frequently speak of an insensible perspiration; that is, the normal, healthful state of the function when there is no disturbing cause. In health, when profuse perspiration is the consequence of severe exercise or hard labor, or when it is caused by the exposure of the individual to a high temperature, great thirst is produced. This can be only subdued by an abundant supply of water. The excessive perspiration has disturbed the process of the conversion of food into blood, and the water is needed to restore the due proportion of things. Even insensible perspiration can not be arrested with impunity. Experiments have been made upon different animals by covering their bodies with a coat of varnish, and the experiments demonstrated the fact that if all perspiratory action is stopped death soon follows. This, in my opinion, is because the blood-making

process—that is, the conversion of food into blood—is totally arrested. The chemical laboratory of the animal body, so to express it, is a wonderfully nice one. Fortunately for man, it can bear up against many disturbing causes; but the sudden stoppage of the functions of organic life is soon fatal. And what is organic life? From birth to death it is nothing else but the conversion of food into blood, and the conversion of blood into the living organized elements of the body when constructive assimilation is at work. During health, as before remarked, I think very little effete matter is removed from the tissues. But life is so variable; a man is well to-day and ailing to-morrow, sometimes weighing a few ounces more, sometimes a few ounces less; so that for the most part some effete matter is being removed from the tissues. This effete matter, in my opinion, is removed from the blood in health by the lungs and liver. When a man is sick effete matter plays a more important part. Many of the functional disorders to which flesh is heir depend upon the irregular or disturbed performance of the process of assimilation or destructive metamorphosis. The human body is capable of enduring this sort of disturbance to a wonderful degree. Many an individual goes through life a chronic sufferer, but yet he lives, and sometimes lives to a good old age. But not so with the process of digestion—of the conversion of the food into blood. Arrest this, and death is soon the inevitable consequence. This is the light in which I consider perspiration. I think that many important therapeutical consequences follow my interpretation of this law of nature.

I have thus briefly referred to the excretory functions of three organs—the liver, the skin, and the kidneys. These excretions are removed from the food before it becomes blood, or in the processes by which it is converted into blood. I have not yet referred to the excretory functions of the liver when that organ is acting upon effete matter. But there is another result of these and the other glandular actions which are concerned in the conversion of food into blood—this is the production of animal heat.

CHAPTER VI.

ANIMAL HEAT.

I now copy extensively from standard authors, that the general reader may understand clearly and distinctly what they teach as to the production of animal heat. We copy first from Dr. Dalton: "According to Lavoisier, the oxygen taken into the lungs was supposed to combine immediately with the carbonic acid and to be at once returned under that form to the atmosphere; the same quantity of heat resulting from the above process as would have been produced by the oxidation of a similar quantity of carbon in wood or coal. Accordingly he regarded the lungs as a sort of stove or furnace by which the rest of the body was warmed through the medium of the circulatory fluid." "It was soon found, however, that this view was altogether erroneous; for the slightest examination shows that the lungs are not perceptibly warmer than the rest of the body; and that the heat-producing power, whatever it may be, does not reside exclusively in the pulmonary tissue. Furthermore, subsequent investigations showed the following very important facts, which we have already mentioned, viz. that the carbonic acid is not formed in the lungs, but exists in the blood before its arrival in the pulmonary capillaries; and that the oxygen of the inspired air, so far from combining with the carbon in the lungs, is taken up in solution by the blood globules and carried away by the current of the general circulation. It is evident, therefore, that the oxidation or combustion of the blood, must take place, if at all, not in the lungs, but in the capillaries of the various organs and tissues of

the body. Liebig accordingly adopted Lavoisier's theory of the production of animal heat with the above modification. He believed the heat of the animal body to be produced by the oxidation or combustion of certain elements of the food while still circulating in the blood; these substances being converted into carbonic acid and water by the oxidation of their carbon and hydrogen, and immediately expelled from the body without ever having formed a part of the solid tissues. He therefore divided the food into two different classes of alimentary substances, viz: 1. The nitrogenous or plastic elements which are introduced in comparatively small quantity, and which are to be actually converted into the substance of the tissues, such as albumen, muscular flesh, etc.; 2. The hydrocarbons or respiratory elements, such as sugar, starch, and fat, which, according to his view, are taken into the blood simply to be burned, never being assimilated or converted into the tissues, but only oxidized in the circulation, and immediately expelled, as above, under the forms of carbonic acid and water. He therefore regarded these elements of the food as so much fuel, destined simply to maintain the heat of the body, but taking no part in the proper function of nutrition."

Objections to this theory:

"First. It is not at all necessary to regard the evolution of heat as dependent solely on direct oxidation. This is only one of its sources, as we constantly see in external nature.

"Second. In vegetables there is an internal production of heat as well as in animals; a fact which has been fully demonstrated by the experiments of Dutrochet and others, already described. In vegetables, however, the absorption of oxygen and exhalation of carbonic acid do not take place, excepting to some extent, during the night. On the contrary, the diurnal process in vegetables, it is well known, is exactly the reverse of this. Under the influence of the solar light they absorb carbonic acid and exhale oxygen. And it is exceedingly remarkable that in Dutrochet's experiments he found that the evolution of heat in plants was always accompanied by the disappearance of carbonic acid and the exhalation of oxygen. Plants, which in the daylight exhale oxygen and

evolve heat, if placed in the dark immediately begin to absorb oxygen and exhale carbonic acid, and, at the same time, the evolution of heat is suspended. Dutrochet even found that the evolution of heat by plants presented a regular diurnal variation, and that its maximum of intensity was about the middle of the day, just at the time when the absorption of carbonic acid and the exhalation of oxygen are going on with the greatest activity. The proper heat of plants, therefore, can not be the result of oxidation or combustion, but must be dependent upon a different process.

“Third. In animals the quantities of oxygen absorbed and of carbonic acid exhaled do not correspond with each other. Most frequently a certain amount of oxygen disappears in the body over and above that which is returned in the breath under the form of carbonic acid. This overplus of oxygen has been said to unite with the hydrogen of the blood, so as to form water, which also passes out by the lungs; but this is a pure assumption resting on no direct evidence, for we have no experimental proof that any more watery vapor is exhaled from the lungs than is supplied by the fluids taken into the stomach. It is superfluous, therefore, to state that any of it is produced by the oxidation of hydrogen. Furthermore, the proportion of overplus oxygen which disappears in the body, besides that which is exhaled in the carbonic acid in the breath, varies greatly in the same animal, according to the quality of the food. Regnault and Reiset found that in dogs fed on meat the oxygen which reappeared under the form of carbonic acid was only seventy-five per cent of the whole quantity absorbed, while in dogs fed on vegetable substances it amounted to over ninety per cent. In some instances where the animals (rabbits and fowls) were fed on bread and grain exclusively, the proportion of expired oxygen amounted to one hundred and one, or even one hundred and two, per cent; that is, more oxygen was actually contained in the carbonic acid exhaled than had been absorbed from the atmosphere in a free state. A portion of the carbonic acid, at least, must therefore have been produced by other means than direct oxidation.

“Fourth. It has already been shown in a previous chapter that the carbonic acid which is exhaled from the lungs is not primarily formed in the blood, but makes its appearance in the substance of the tissues themselves; and furthermore, that even here it does not originate by direct oxidation, but rather by a process of decomposition similar to that by which it is produced from sugar in its alcoholic fermentation. We understand from this how to explain the singular fact alluded to in the last paragraph, viz. the abundant production of carbonic acid under some circumstances with a comparatively small supply of oxygen. The statement made by Liebig, therefore, that starchy and oily matters taken with the food are immediately oxidized in the circulation without ever being assimilated by the tissues, is without foundation. It never, in fact, rested on any other ground than a supposed probability; and as we see that carbonic acid is abundantly produced in the body by other means, we have no longer any reason for assuming without evidence the existence of a combusive process in the blood.

“Fifth. The evolution of heat in the animal body is not general, as it would be if it resulted from a combustion of the blood, but local, since it takes place primarily in the substance of the tissues themselves. Various causes will therefore produce a local deviation or depression of temperature by modifying the nutritive changes which take place in the tissues. Local inflammations increase very sensibly the temperature of the part in which they are seated, while that of the general mass of the blood is not altered. Finally, it has been demonstrated by Bernard that in the natural state of the system there is a marked difference in the temperature of the different organs and of the blood returning from them. The method adopted by this experimenter was to introduce into the living animal the bulb of a fine thermometer, successively into the blood-vessels entering and those leaving the various internal organs. The difference in temperature in these two situations showed whether the blood had lost or gained in heat while traversing the capillaries of the organ. Bernard found, in the first place, that the blood, in

passing through the lungs, so far from increasing was absolutely diminished in temperature, the blood on the left side of the heart being sometimes a little more, sometimes a little less, than one third of a degree F. lower than on the right side. The slight cooling of the blood in the lungs is owing simply to its exposure to the air through the pulmonary membrane and to the vaporization of water which takes place in those organs. In the abdominal viscera, on the contrary, the blood is increased in temperature. It is sensibly warmer in the portal vein than in the aorta, and very considerably warmer in the hepatic veins than in the portal or vena cava. The blood in the hepatic veins is, in fact, warmer than that of any other part of the body. The production of heat, therefore, according to Bernard's observations, is more active in the liver than in any other portion of the system. As the chemical processes of nutrition are necessarily different in the different tissues and organs, it is easy to understand why a specific amount of heat should be produced in each of them. A similar fact, it will be recollected, was noticed by De-trochet in regard to the different parts of the vegetable organism.

"Sixth. Animal heat has been supposed to stand in a special relation to the production of carbonic acid, because in warm-blooded animals the respiratory process is more active than in those of a lower temperature, and because in the same animal an increase or diminution of the evolution of heat is accompanied by a corresponding increase or diminution in the products of respiration. But this is true of all other excretory products of the body. An elevation of temperature is accompanied by an increased activity of all the nutritive processes. Not only carbonic acid but the ingredients of the urine and the perspiration are discharged in larger quantity than usual. An increased supply of food is also required, as well as a larger quantity of oxygen; and the digestive and secretory processes both go on at the same time with unusual activity.

"Animal heat, then, is a phenomenon which results from the simultaneous activity of many different processes taking place in many different organs, and dependent undoubtedly on different

chemical changes in each one. The introduction of oxygen and the exhalation of carbonic acid have no direct connection with each other, but are only the beginning and ending of a large series of changes in which all the tissues of the body successively or simultaneously take part. Their relation is precisely that which exists between the food introduced into the stomach and the urine discharged by the kidneys. The kidneys require for their nutrition a constant supply of solid and liquid food, which is introduced through the lungs. The disintegration and decomposition of the tissues give rise, on the one hand, to urica, uric acid, etc., which are discharged with the urine, and, on the other hand, to carbonic acid, which is exhaled from the lungs."

Dr. Flint, vol. 3, page 423, says, "What is the relation to calorification of those processes of nutrition which involve the consumption of nitrogenized matter and the production of nitrogenized excrementitious principles?"

"We can not study these phenomena alone, isolated from other acts of nutrition. We may confine an animal to a purely nitrogenized diet, and the heat of the body will be maintained at the proper standard, but at all other times there is a certain quantity of non-nitrogenized matter (sugar and, perhaps, fat) produced in the system, which is only formed to be consumed. We may starve an animal, and the temperature will not fall to any great extent until a short time before death. Here we may suppose that the process of deposition of nutritive matter in the tissues from the blood is inconsiderable as compared with the transformation of the substance of these tissues into effete matter; and it is almost certain that non-nitrogenized matter is not produced in the organism in quantity sufficient to account by its destruction in the lungs for the carbonic acid exhaled. It seems beyond question that there must be heat evolved in the body by the oxidization of nitrogenized matter. When the daily amount of food is largely increased for the purpose of generating the immense amount of heat required in excessively cold climates, the nitrogenized matters are taken in greater quantity as well as the fats, although their increase is not in the same proportion.

When, however, we endeavor to assign to nitrogenized matters a definite proportion of heat-producing power, we are arrested by a want of positive knowledge with regard to the metamorphoses which these principles undergo, and it is equally impossible to fix the relative calorific value of the deposition of new material in repair of the tissues and the change of their substance into effete material by disassimilation."

"From these facts, and other considerations that have already been fully discussed under different heads, it is evident that the physiological metamorphoses of nitrogenized matter bear a certain share in the production of animal heat, although, in connection with inorganic matter, their chief function seems to be the repair of the tissues endowed with the so-called vital properties."

As far as the destination of amylaceous, saccharine, and fatty matter of the food is concerned, we only know that they are incapable of themselves of repairing muscular tissue, and that they can not sustain life. They are never discharged from the body in health in the form under which they enter, but are in part or completely destroyed in nutrition. They are completely destroyed in persons who, from habitual muscular exercise, have very little adipose tissue. When their quantity in the food is large they are of necessity entirely consumed, but may be deposited in the form of adipose tissue. This, however, may be made to disappear by violent exercise or under an insufficient diet.

There can be no doubt that the non-nitrogenized class of alimentary principles is craved by the system in long-continued exposure to extreme cold. This is particularly marked with regard to the fats. In all cold climates fat is a most important article of food; and in excessively cold regions, while the nitrogenized elements are largely increased, there is a very much larger proportional increase in the quantity of fat. These facts are very significant. If the non-nitrogenized elements of food—which are not always indispensable, though very often necessary articles—do not form tissue, are not discharged from the body, and are consumed in some of the processes of nutrition, it would seem that this change must involve the production of carbonic

acid, perhaps also of water, and the evolution of heat. It is so difficult to ascertain the exact quantities of carbonic acid, watery vapor, etc. thrown off by the lungs, skin, and other emunctories, and to estimate the exact amount of heat produced and lost, that it is not surprising that calculations of the calorific power of different articles of food should be frequently erroneous, particularly as we have no means of knowing the exact calorific value of the nitrogenized principles."

"Though we may assume that the non-nitrogenized elements of food are particularly important in the production of animal heat, and that they are not concerned in the repair of tissue, it must be remembered that the animal temperature may be kept at the proper standard upon an exclusively nitrogenized diet, and we can not indeed connect calorification exclusively with the consumption of any single class of principles, nor with any single one of the acts of nutrition."

The extracts given above show, I think, clearly and distinctly, the views entertained by physiologists as to the production of animal heat. I do not pretend to object to the correctness of the doctrines taught, except in one particular. I fully agree with Dr. Dalton in his concluding remark, that "animal heat, then, is a phenomenon which results from the simultaneous activity of many different processes taking place in many different organs, and dependent undoubtedly on different chemical changes in each one." The method of its production has not yet been satisfactorily ascertained, scientific investigators themselves being judges. The doctrine to which I object is that expressed by Dr. Dalton in the following sentences: "The tissues require for their nutrition a constant supply of solid and liquid food which is introduced through the lungs. The disintegration and decomposition of the tissues give rise, on the one hand, to urea, uric acid, etc., which are discharged with the urine, and, on the other hand, to carbonic acid which is exhaled from the lungs."

I have so often expressed my disagreement with physiologists as to the doctrine they teach as to what becomes of the food after it is absorbed, that I will not here again repeat my objections.

That the union of oxygen with coal and wood produces combustion, no one denies; but the proximate cause of the combustion is ignition applied in some way. Animal heat is coexistent with vitality. As a lucifer match causes coal or wood to burn, so vitality causes animal heat to be produced in the conversion of food and drinks into blood; and in the conversion of this blood into organized tissues when the tissues are formed. In animals, as well as in trees and plants, vitality is ever operating, and is the proximate cause of the union of oxygen with the food within the living organism. But there is another product consequent upon the union of oxygen with the food of plants and animals. That product is MOTION. Animal heat is coexistent with, and inseparable from, motion. The same is true in the life of plants and trees. The temperature of the latter is but a few degrees above the surrounding atmosphere, except when they are growing and flowering. During this period in plants heat is wonderfully developed.

“The rise of the thermometer when applied to the spadix of the arum at the time of flowering has long been known. Senebier found the temperature 7° higher than the surrounding atmosphere. Hubert, in experiments on the arum cordifolium, in the Isle of France, found the thermometer rise from 66° , the temperature of the surrounding atmosphere to 111° , when placed in the center of the spadix, and in others to 121° , thus indicating a difference in one case of 45° and in others of 55° . The greatest difference was observed to be in the morning. The accurate experiments of Brongniart have rendered it more than probable that in all cases of flowering heat is liberated, although from the structure or size of the flower it may be impossible to detect it by instruments.” “It is well known that during the flowering of plants oxygen is absorbed, and in some cases this absorption has amounted to thirty times the volume of the subject of experiment in twenty-four hours; during this time carbonic acid is given off. These phenomena plainly indicate the cause of the heat during the period of fertilization.” All plants give off carbonic acid and absorb oxygen while flowering. The instances

referred to above show an excessive development of heat, consequent upon an enormous consumption of oxygen. It may be said to be the exaggeration of this law of vegetable life. Carbonic acid, water, organic and inorganic substances in the soil constitute the food of plants. The union of the oxygen of the atmosphere with the organic part of this food develops growth; the flowering and seed-producing functions, heat and motion." (Botany of the Southern States, by Prof. John Darby, A. M.)

Boussingault says, "Whatever be its constitution and physical properties, land will be fertile only as it contains organic matter in a more or less advanced state of decomposition. There are favored soils in which this matter is found in abundance, but there are others in which it exists only in a very small quantity. To become productive these lands require the intervention of manure; for this there is no substitute either in the labor which breaks them up, nor in the climate which powerfully promotes their fecundity, nor in the alkalies and salts which are such powerful auxiliaries to vegetables." That is to say, plants must always be supplied with organic elements of food, as well as with an abundance of water, to enable them to grow vigorously, to flower profusely, and bear seed abundantly. Plants give off carbonic acid during the night and absorb oxygen. During the day, while exposed to solar light and heat, they absorb carbonic acid and give off oxygen; though Soissure maintained that plants give off carbonic acid at all times. Some plants, as mushrooms, which are of very rapid growth, certainly do give off carbonic acid while exposed to both the light and heat of the sun. All plants give off carbonic acid more or less abundantly while flowering. It is the active period of their organic life. Where the largest amount of heat is developed, and the greatest quantity of oxygen taken up by them, no doubt an excessively rapid motion in the sap or blood of the plant occurs. I do not know that any experiments have been made to prove how excessively rapid this flow of the sap may sometimes be; but even during winter it continues. Dr. Carpenter says, "Some motion of the sap takes place even in winter; and, as the earth at a few feet below the

surface preserves a very uniform temperature, it is not improbable that the transmission of fluid derived from it through the stem, may have an influence on the state of the latter. A supposition which is countenanced by the fact that the temperature of the interior of a large trunk and that of the soil four feet below the surface (which may be regarded as the medium depth of roots) bear a very close correspondence." But a very important part of the food of plants is water. The soil may be ever so rich in organic materials, yet without an abundant supply of water they will not grow vigorously. Especially is this the case when they are flowering and producing their seed. This was well known to the Asiatics centuries ago. In the story of the three calenders, sons of kings, and the five ladies of Bagdad, when the third calender had been left alone in the enchanted castle, he visited the gardens and orchards which surrounded it, and thus described what he saw: "I ought not to forget, madam, to acquaint you that this delicious orchard was watered after a very particular manner; there were channels so artificially and proportionably digged that they carried water in abundance to the roots of such trees as wanted it, for making them produce their leaves and flowers. Others carried it to those that had their fruit budded; some carried it in lesser quantities to those that had their fruit growing big; and others carried only so much as was just requisite to water those which had their fruit come to perfection, and only wanted to be ripened. They exceeded the ordinary fruits of our gardens very much in bigness; and lastly, those channels which watered the trees whose fruit was ripe had no more moisture than just what would preserve them from withering."

It will not be contended by any one, I think, that effete matter is produced in plants or trees. Their food is converted into sap, and this sap into the texture of the plant or tree. The honey found in the flowers of many of them is a secretion, not an excretion. Carbonic acid may be said to be the excretion of the plant, but it is an excretion produced in the conversion of the sap or food of the plant into the texture of its organism. It

was not a part of that organism, nor was it separated from it as effete matter. It is because physiologists suppose that the food and drinks all go to form organized tissues, and that the excretions thrown off from an animal body were a part of these organized tissues, and that this law obtains at all times in animal bodies, that they have not been able to understand the source of animal heat. We have said that wood and coal must be ignited before they will burn. Not so in animal bodies. Animal heat was an inherent part of the bioplasm existing in the germs of life, whether those germs be animal or vegetable. Animal heat was inherited by every living thing. We have seen that vegetative life depends upon inorganic matter, organic matter or manures found in the soil, upon a more or less abundant supply of water, according to the conditions of growth, of flowering, of fructification, and an abundant supply of solar light and heat. The substances taken up by the roots of trees or vegetables constitute their food. Mr. Darby has shown the rapid and wonderful development of heat in flowering plants; he shows also the enormous amount of oxygen absorbed by some plants within twenty-four hours, during which time carbonic acid is given off. The passage of the food into and through the tree and flowering plant, and its conversion into leaves, blossoms, and wood, must be rapid in proportion to the absorption of oxygen, the throwing off of carbonic acid, and the development of heat. Heat and motion always coexist. I believe it is admitted by all physiological thinkers that when the cause of a thing is discovered or ascertained which is sufficient to explain the facts or consequences, we should therewith be content. All we have to do is to satisfy ourselves that the explanation includes all the facts. In plants as in animals we have vitality to begin with. It is the proximate cause of all the organic movements in the body, whether it be that of a tree or of a man. The temperature of both plants and animals varies at different periods of the day. This depends upon solar influence. In the higher order of animals it depends also upon a sort of periodicity of the circulation of the blood.

As to the part nitrogen performs in the growth of plants, Berzelius says, "How the nitrogen in plants is obtained is still more obscure than that of carbon. Plants and animals live enveloped by the air, which, in its ordinary state of moisture, contains seventy-eight and a half per cent of nitrogen by volume. Nothing is easier than to suppose that the nitrogen in living bodies is derived from the air. But even if this supposition is correct, there is in it a mystery which is not yet solved. Nitrogen, as we know, is found in the air in an allotropic modification of a character similar to that found in silica, chromium, titanium, etc., and in which it seems destitute of any disposition to combine with any other bodies. If then their nitrogen is derived from the air, there must be in nature some means by which, under circumstances of which we have no certain knowledge, it is changed from the indifferent allotropic modification into the other state in which it enters into combination."

"Liebig supposes that the nitrogen in plants originates only from ammonia, which is the only form in which nitrogen can be assimilated by them, and that in this form it enters all nitrogenous bodies. As he found a small quantity of ammonia in rain-water, he supposes that the amount of ammoniacal gas in the atmosphere is so great that rain-water every year transfers from the air to the earth far more ammonia than is necessary, even though there be taken from the soil rich harvests of grain, the seeds of which contain a large per cent of nitrogen in proportion to their weight. Mulder has, however, shown that Liebig's hypothesis is wholly arbitrary in regard to the quantities of ammonia which he himself finds in the air. Experiments by Grazer show that rain-water which has been collected after long-continued dry weather, and that after many days abundant rain, contains about the same quantity of ammonia; but it is always a mere trace, which is far less than Liebig assumes in his estimates. Opposed to Liebig's theory are the experiments of Bous-singault, according to which herbaceous plants did not arrive at maturity from their seeds in earth which contained no nitrogenous matter. The same results he found with the leguminous

plants, as peas, beans, and clover. Wheat and oats withered after flowering without maturing their seeds, and the dry plants contained exactly the quantity of nitrogen contained in the seeds from which they were raised. In these cases the nitrogen is wanting for the development of the seed, which accordingly miscarry. Peas, beans, and clover grow with little luxuriance in a soil devoid of nitrogenous matter; but on planting more vigorous seeds an analysis shows that the amount of nitrogen in the ripe plants is double as great as in the seeds which produced them. It seems from this to be evident that the first kind of plants must receive their nitrogenous matter through the roots, while the latter can assimilate the nitrogen diffused through the atmosphere. Wungmann and Pollstroff planted cresses and the *lepidum sativum* on finely-divided platinum wire, and supplied them with water free from ammonia. The plants grew luxuriantly and produced well-developed seeds, the nitrogen of which must have been obtained from the atmosphere."

Dr. Flint, vol. 3, page 424, 425, says, "From these facts and other considerations that have already been discussed under different heads, it is evident that the physiological metamorphoses of nitrogenized matter bear a certain share in the production of animal heat, although in connection with inorganic matter their chief function seems to be the repair of the tissues endowed with the so-called vital properties." "What is the relation of the consumption of non-nitrogenized matter to the production of animal heat?"

"It has been pretty clearly shown that both sugar and fat are actually produced in the organism, even when the diet is strictly nitrogenized in its character; but we will only consider the non-nitrogenized elements introduced into the body, assuming that the principles of this class appearing *de novo* in the organism are the result of transformation of non-nitrogenized substances." "As far as the destination of the amylaceous, saccharine, and fatty elements of the food are concerned, we only know that they are incapable of themselves of repairing muscular tissue, and that they can not sustain life. They are never discharged from the

body in health in the form under which they enter; but are in part or completely destroyed in nutrition. They are completely destroyed in persons who, from habitual muscular exercise, have very little adipose tissue. When their quantity in the food is large, they are not of necessity entirely consumed, but may be deposited in the form of adipose tissue. This, however, may be made to disappear by violent exercise, or under an insufficient diet." There can be no doubt that the non-nitrogenized class of alimentary principles is craved by the system in long-continued exposure to extreme cold. This is particularly marked with regard to the fats. "In all cold climates fat is a most important element of food; and in excessively cold regions, while the nitrogenized elements are largely increased, there is a very much larger proportional increase in the quantity of fat. These facts are very significant. If the non-nitrogenized elements of food—which are not always indispensable, though often very necessary articles—do not form tissue, are not discharged from the body, and are consumed in some of the processes of nutrition, it would seem that their change must involve the production of carbonic acid, perhaps also of water, and the evolution of heat. It is so difficult to ascertain the exact quantities of carbonic acid, watery vapor, etc. thrown off by the lungs, skin, and other emunctories, and to estimate the exact amount of heat produced and lost, that it is not surprising that calculations of the calorific power of different articles of food should be frequently erroneous, particularly as we have no means of knowing the exact calorific value of the nitrogenized principles." "Though we may assume that the non-nitrogenized elements of food are particularly important in the production of animal heat, and that they are not concerned in the repair of tissue, it must be remembered that the animal temperature may be kept at the proper standard upon an exclusively nitrogenized diet; and we can not indeed connect calorification exclusively with the consumption of any single class of principles, nor with any single one of the acts of nutrition." In those transformations within the living body wherein the action of nitrogen is concerned, motion is produced, and heat is one of

the results as much so as in the union of the oxygen of the atmosphere with the carboniferous portions of the food. The effect produced by both is action, vital action, if you please. This action originates the secretory and excretory motion of the glands, one of the consequences of which is animal heat.

In the extended extracts from the works of the distinguished authors referred to, it will be observed that in the life of plants, as well as in that of animals, when there is a large consumption of food there is increased action with a corresponding development of animal heat. These are the constant quantities. Physiologists may differ as to the part played by oxygen or nitrogen in the vital organism of plants or animals. I differ from them in this: I maintain that heat is developed in the animal organism during health through the various acts or functions, secretory and excretory, by which the food is converted into blood. Whatever may be the ultimate uses of food composed of sugar and fats, one thing, in my opinion, is certain—they must first be converted into blood. The fact that they are both sometimes found floating in the blood and in the lymph, only shows that their conversion into blood is more difficult than that of food composed of nitrogenous substances. This is applicable, perhaps, more particularly to the fats. This difficulty requires increased labor; that is, greater action or motion.

The result of this increased motion is a larger evolution of animal heat. By this wonderfully beneficent law men are enabled to maintain a comfortable temperature while exposed to the most intense cold. No doubt a vastly larger amount of heat is generated in their bodies than is produced in the bodies of men living in the tropics, who live on fruits and vegetables; but the amount of heat does not arise above the standard of health, say 98° F., because of the constant escape of heat from their bodies. Physiologists have observed that the blood is warmer in some parts of the body than others. M. Claude Bernard proved by experiment that the blood in the portal vein is warmer than that in the aorta, and that the blood in the hepatic veins is several

degrees warmer than that in the portal. It does not appear how long after eating these experiments were made. If shortly after, or within a few hours, there is increased action in the secretory glands of the intestines, and also highly increased action in the spleen which receives into itself and pours out into the portal vein a large amount of blood. In both instances there would be increased action or motion with a corresponding development of heat. The same is true of the liver. There is a quadruple or quintuple circulation in the liver—that of the hepatic artery, portal veins, biliary ducts, hepatic veins, and lymphatic vessels.

The liver no doubt performs more labor than any gland within the body, the lungs, perhaps, excepted. Hence the increased motion with a corresponding increase in the temperature of the blood. The blood found in the pulmonary veins, coming from the lungs, would no doubt be warmer than the blood of the pulmonary artery entering these organs but for its exposure to the atmosphere and the escape of watery vapor as has been observed or remarked upon by different physiologists. The true explanation, I think, why the temperature of the lungs and of the blood found in the pulmonary veins is not as high as the temperature of the liver and of the blood found in the hepatic veins, is this, viz. when solids pass into a liquid form, and when liquids pass into a watery or gaseous form, the heat evolved becomes latent. It escapes from the lungs with the carbonic acid and watery vapor expired as latent heat. By a wonderfully beneficent law of the omniscient Creator this latent heat so removed from the body is made to protect and preserve the life of animals and of man in extremely cold climates; it is continually observed in such climates that the beard of man and the hair around the nostrils and mouths of other animals is covered with frost when the men and animals are exposed to intense cold. The watery vapor expired has frozen, and in the act of freezing the latent heat contained in it is given out. The heat so released lessens the intensity of the cold of the atmosphere surrounding the mouth, nostrils, and face, which are less protected than the

other parts of the body. There is a reason, then, why the lungs should throw off heat in a latent form.

Herein is a law of organic life developing a physical or chemical law, and the next link in this chain of causation, when the expired vapor is frozen, is that the physical or chemical law develops a vitalizing or life-preserving effect. Scientists, who deny that design necessitates an intelligent Creator, will find it difficult to explain how a dynamic force, acting upon inorganic matter, comes to unite with a law of animal life in effecting so beneficent a result, a result upon which the health and life of thousands of men and animals (in the Arctic regions) depend.

But whenever secretory action is going on increased motion attends it, with its constant companion, increased heat. This applies to every gland in the body. Besides those referred to above, it applies particularly to the skin, and also to the lymphatics, which are so widely distributed throughout the animal economy.

When a man or horse exercises laboriously they become heated. In this case the circulation of the blood is more rapid; a greater volume of blood is continually passing through the heart; that is to say, there is increased motion. But it may also be correctly said the inspirations are deeper and fuller. A larger amount of oxygen is consumed, and a larger amount of carbonic acid is thrown off. Herein, no doubt, the theory of Liebig lies; an increased quantity of oxygen unites with the carbonaceous portions of the food floating in the blood or with the carbon in the blood, the result being increased animal heat. But it must be borne in mind that plants absorb oxygen and give off carbonic acid during the night when their temperature is lowest, and give off oxygen and absorb carbonic acid during the day when their temperature is highest, except while flowering, when it is said they absorb oxygen and give off carbonic acid in larger quantities under the influence of a midday sun. All that we positively know is that the secretory and excretory organs are most actively engaged when the largest amount of heat is evolved.

When we come to consider pathological conditions—take a

fever case, for instance, where the patient eats nothing and the system is feeding on itself; that is to say, effete matter affords the material for all the excretions—we have greatly increased motion in the circulation with a corresponding increase in the temperature of the body. On the other hand, examine a case with an ague. Here we have greatly weakened action of the heart, a smaller and smaller quantity of blood is passing through that organ and through the lungs. It is congested within the cavities, particularly in the portal circle of the abdominal cavity. Here we have a lowering of the temperature until the limbs and parts of the body of the patient become icy cold. The most obvious fact in this condition is the diminished and diminishing motion or enfeebled circulation of the blood. This applies to the cold stage of congestive fever and to the collapse of cholera. The question of congestion and hyperemia, with diminished and increased temperature of the body, will be fully considered in a separate chapter. But it may be said that in the cold stage of the maladies referred to the nervous system is involved.

As to the connection of the nervous system with the development of animal heat, Dr. Flint says, "Helmholtz, DuBois Raymond, and others have noted certain conditions which modify the rate of nervous conduction. One of the most prominent of these, first observed by Helmholtz, is due to modifications in the temperature. By a reduction of temperature in the frog, at least, the rate is very much reduced, and at 32° it is not more than one tenth as rapid as at 60° or 70°. Marcy has also noted that the rate is sensibly reduced by fatigue of the muscle." "There is little to note under this head except the fact that functional activity of the nerves produces an amount of elevation of temperature in their substance which can be distinctly demonstrated by sufficiently delicate thermometric apparatus." (Page 104, vol. 4.) "In 1854 Bernard repeated the experiments of Pourfour DuPéttit dividing the sympathetic in the neck on the one side in a rabbit, and noted on the corresponding side of the head and ear increased vascularity and an elevation in temperature amounting to from 7° to 11° F." (Page 437.)

"It has been shown by direct experiments upon living animals that local variations in the circulation, independent of the action of the heart, actually take place, and that they are of great importance in special functions; and there are numerous instances of such action which can only take place through the nervous system. The phenomena of blushing and pallor from mental emotion are familiar examples." (Flint's Physiology, vol. 4.)

"That local variations in the circulation, independent of the action of the heart," may occur, and do occur, when a nerve is divided, as in the experiment performed by Bernard, there is, we suppose, no doubt; but that blushing and sudden pallor occur independent of the heart's action we deny, the individual being in a state of good health. Numerous experiments have been made to ascertain precisely the rapidity of the circulation. It is wonderfully quick, sufficiently so for both pallor and blushing to be produced by an altered condition in the heart's action. The ancients supposed the heart to be the seat or organ of the emotions and affections. To-day, in everyday life, men constantly speak of the heart in the same sense. It is because men have observed that the heart's action is constantly affected by every emotion, whether that emotion be produced by seeing or hearing, or even by our own reflections without seeing or hearing any thing. Both physiologists and pathologists are too much disposed to overlook the heart's action and to seek for an explanation in the action of the capillaries, simply, we think, because these are more particularly the subject of microscopic examinations. Were the ear applied over the heart or the finger to the radial artery at the time that a maiden blushes we do not doubt it would be discovered that there was increased force in the heart's action, and also in the case of sudden pallor that there would be diminished force; the one of these states is accompanied by increased, the other by decreased motion with corresponding higher and lower temperature.

Without stopping here to notice particularly M. Bernard's experiment, we will only call the attention of the reader to the fact that the increased temperature observed is dependent on

increased motion in the circulation of the blood through the capillaries in the part which was supplied by the divided nerve. Here we find that although the increased capillary circulation may be independent of the heart's action, the increased temperature is not independent of increased motion. In both physiological and pathological conditions the temperature of the body of the living animal is so dependent upon increased or diminished motion in the circulation of the blood and of the nervous fluid, that it is inseparable from it.

In pathological conditions heat is generated in some part of the body so long as destructive metamorphosis is going on in the living animal. The greater the motion the greater the amount of heat produced. The American Indians kindle a fire by rubbing two hard and dry sticks together. The sticks would have remained an indefinite time surrounded by the oxygen of the atmosphere. It was friction or motion which ignited them.

CHAPTER VII.

THE LIVER.

"The liver is situated just below the diaphragm in the right hypocondriac region, and is the largest gland in the body, weighing, when moderately filled with blood, about four and a half pounds. Its weight is somewhat variable, but it is stated by Sappey that in a person of ordinary adipose development its proportion to the weight of the body is about one to thirty. In early life the liver is relatively larger, its proportion to the weight of the body in the new-born child being as one to eighteen or twenty."

"The liver is covered externally by the peritoneum, folds or duplicatures of this membrane being formed as it passes from the surface of the liver to the adjacent parts. These constitute four of the so-called ligaments that hold the liver in its place."

"The proper coat of the liver is a very thin, but dense and resisting, fibrous membrane adherent to the substance of the organ, but detached without much difficulty, and very closely united to the peritoneum. This membrane is of variable thickness at different parts of the liver, being especially thin in the groove for the vena cava. At the transverse fissure it surrounds the duct, blood-vessels, and nerves, and penetrates the substance of the organ in the form of a vagina or sheath surrounding the vessels and branching with them. This membrane, as it ramifies in the substance of the liver, is called the capsule of Glisson."

"At the transverse fissure, the portal vein, collecting the blood from the abdominal organs, and the hepatic artery, a branch of the celiac axis, penetrate the substance of the liver, with the

hepatic duct, nerves, and lymphatics all enveloped in the fibrous vagina or sheath known as the capsule of Glisson."

"The portal vein is by far the larger of the two blood-vessels, and its caliber may be roughly estimated at from eight to ten times that of the artery."

"The blood which has circulated through the capillaries of the stomach, spleen, pancreas, and intestines, is collected by the roots of the corresponding veins and discharged into the portal vein, which enters the liver at the transverse fissure of the organ."

"Immediately upon its entrance the portal vein divides into two branches, right and left, which supply the corresponding portions of the liver, and these branches successively subdivide into smaller twigs and ramifications until they are reduced to the size, according to Kölliker of $\frac{1}{1200}$ of an inch in diameter."

"The portions of hepatic substance included in this way between the terminal branches of the portal vein are termed the 'acini' or 'lobules' of the liver, and the terminal venous branches occupying the spaces between the adjacent lobules are 'interlobular veins.'"

"In the spaces between the lobules we also find the minute branches of the hepatic artery, and the commencing rootlets of the hepatic ducts."

"The hepatic duct is formed by the union of two ducts, one from the right, and the other from the left lobe of the liver. It is about an inch and a half in length, and joins at an acute angle with the cystic duct to form the ductus communis choledochus. The common duct is about three inches in length, of the diameter of a goose-quill, and opens into the descending portion of the duodenum. It passes obliquely through the coats of the intestine, and opens into its cavity in connection with the principal pancreatic duct. The cystic duct is about an inch in length, and is the smallest of the three canals."

"The gall-bladder is an ovoid or pear-shaped sac about four inches in length, one inch in breadth at its widest portion, and capable of holding from an ounce to an ounce and a half of fluid."

The above extracts are taken from Flint and Dalton. The hepatic duct is made up of the biliary ducts which come from all parts of the liver. In this respect they resemble the lymphatic system, which, commencing in all parts of the body, finally terminates in the thoracic duct and right lymphatic duct. Of the nerves going to the liver and the lymphatics of that organ it is unnecessary in this connection for us to speak. We purposely avoid attempting to notice the microscopic anatomy of the liver, or even to allude to the learned researches made in reference to its microscopic anatomy. There are six systems of circulation in the liver: the hepatic artery and portal vein, which carry blood to the liver; the capillary system of blood-vessels in it; the hepatic veins, which carry the blood from the liver; the biliary ducts, which together form the ductus hepaticus, and empty the bile into the duodenum; and the lymphatics, which empty their contents into the thoracic duct. Besides these arteries, veins, capillaries, lymphatics, and biliary ducts there are the nerves going to the liver which modify or control the action of them all. The hepatic artery performs the same office for the liver that the coronary arteries perform for the heart, as also the bronchial arteries do for the lungs, and enable it to perform its varied and laborious functions. The blood carried to the liver by the hepatic artery may be subjected to some alteration in its passage through that organ, as all arterial blood is changed in its passage through the capillary system into the venous system in every part of the body. But arterial blood is not carried to the liver specially to be changed, as is the great volume of blood carried to that organ through the portal vein. The lymphatics of the liver are not unlike the lymphatics of other parts of the body. The same may be said of its capillary blood-vessels and of its nerves. The remarkable peculiarity in the circulatory system of the liver is this, the arterial blood conveyed from the heart to the abdominal organs is not returned directly to the heart by the veins, as is the case in all other parts of the body, but it is taken up by a system of veins which do not go to the heart at all. These are the portal veins. These veins originate in the

stomach, in the intestines, small and large, from the pyloric orifice of the stomach to the anus; from their mucous as well as from their serous membrane or peritoneal coat; from the mesentery and mesenteric glands; from the pancreas, and from the spleen, all of which empty into the portal vein. When an individual is in a state of perfect health these veins may be said to carry blood comparatively pure into the great portal vein. This blood, however, is deprived of many of the constituents it contained when it left the heart. That which goes to the stomach supplies that organ with healthy mucus and also its gastric juice. That which goes to the intestines supplies the mucus which lines their inner coats, and also the intestinal juices secreted by the innumerable glands formed within the intestinal tube. That which goes to the mesenteric glands supplies the secretions which are mixed up with the chyle as it passes through them on its way to the thoracic duct. That which goes to the pancreas supplies the pancreatic juice. The blood which left the heart, I say, supplies all of these secretions before it enters the veins which convey it to the liver. In the varied functions which this blood is thus made to perform, it is deprived of many of its constituents and is meager in vital power.

This is its condition in health. What must be its condition when subjected to all the morbid or pathological conditions which may exist, and which constantly do exist, in the alimentary canal from the termination of the esophagus to the anus? What must be its condition on returning from disordered or diseased mesenteric glands, or from pancreas functionally or organically disordered. It would be difficult to name the malady to which flesh is heir that does not affect injuriously some portion of the alimentary canal with its appendages. It may be a malady gradual in its approach and slow in its progress, as dyspepsia; or it may be a sudden seizure, which pours out gallons of rice-water, so-called, as cholera. It may be dysentery, with its mucopurulent and bloody discharges; or it may be typhoid fever with its ulcerated glands, and patches of glands, in the small intestines; or it may be peritoneal inflammation, with its sudden and

destructive tendencies; or it may be the piles, which hold for a long time in its enlarged and pouchy veins disordered blood. All these, and other maladies I might name, affect the blood which has to be returned to the liver through the portal vein. Such is the condition of the blood upon which the liver has to act when the individual is sick. And yet, remarkable to state, there are physiologists who maintain that the liver does not separate effete matter from the blood. Dr. Flint says, "Inasmuch as cholesterin is the *only* excrementitious principle as yet discovered in the bile, bearing the same relation to this fluid that urea does to the urine, it is evident that the ideas of the physiologist with regard to any excretory function of the liver must have been very indefinite before the relations of cholesterin had been determined." We will refer to this again, and also to the absurd notion that the kidneys in health separate effete matter from the blood. It must always be borne in mind, and I can not too frequently repeat it, that physiologists define effete matter to be something which has been a part of living organized tissue, as flesh, nerves, brain, and bone.

But to return, it is no less astonishing to me that after all the patient, laborious, and learned researches, made by so large a number of eminent physiologists distinguished for their knowledge in many departments, there has not been one among them to discover the offices and uses of the spleen. I have stated, just above, the alterations to which the blood is subjected, even in healthy individuals, in its journey from the heart to the liver through the abdominal organs. I have also briefly stated how it must be mixed up with corrupt and vitiated secretions, when the abdominal organs through which it has to pass are functionally or organically disordered or diseased. It is to correct this altered or diseased condition of the blood that the spleen stands ready to pour continually into the portal vein fresh arterial blood revived, if I may so express it, by its passage through that ductless gland. I know that the spleen has been removed from dogs and yet they lived; but I also know that there never was a man with a large or diseased spleen in his body who enjoyed good health.

In a former chapter, while I was following the food after it had entered the blood-vessel system, I pursued it up to the spleen, where the connection of my subject compelled me to leave it. I must now return to it. As to the alterations in the blood in the passage through the spleen, the only positive conclusions, to which physiologists have come, can be expressed in a single sentence: "Béclard and Gray observed a marked increase in the fibrin and albumen in the blood of the splenic veins." All physiologists admit, indeed they all teach, that fibrin and albumen are two of the most important constituents of the blood. They are two of the elements out of which the red corpuscles are formed. Could there be two any more important elements poured into the altered or vitiated blood coming from the abdominal organs? It seems, to my common-sense view, an all-sufficient function for the spleen to be continually pouring into the portal vein these highly organized elements of vitality.

Let it be borne in mind, as stated in a former chapter, that the increased quantity of albumen and fibrin found in the splenic veins is formed from the food floating in the blood as it passes through this elaborately organized ductless gland. The liver has three important functions to perform. The first is to assist in converting the food into blood; the second is to remove effete matter from the blood; and the third to convert effete matter, and that portion of the food which ought to be separated from it, into bile. The first of these functions of the liver, physiologists ignore altogether. They all seem to suppose that so soon as the food is taken up from the stomach and intestines by the venous absorbents, and by the lacteals, it becomes blood. After this absorption they treat of it and speak of it only as blood. So far has this notion taken possession of the minds of the physiologists that when M. Claude Bernard discovered sugar in the hepatic veins, he proclaimed to the world of physiologists that he had discovered that the liver possessed a glycogenic or sugar-producing function. Some of the ablest physiologists in the world are now engaged in microscopic and chemical analyses and examinations to prove or to disprove the

truth of this discovery. When it is considered that more than one half of the food of more than one half of the human race is converted into glucose, one of the forms of sugar, before it enters the portal veins, is it not passing strange that physiologists would not expect to find sugar in the liver? One of the functions of that organ is to convert this glucose into blood, and in the process of this conversion what it separates from the glucose it forms into bile. M. Bernard divided the eighth pair of nerves in the neck of a rabbit and found, he says, that the glycogenic function, or ability, of the liver was greatly lessened. This he imagines to be "confirmation strong as proof of holy writ" of the truth of his theory. Did it not occur to him that the pneumogastric nerves are distributed to the stomach and duodenum, and, perhaps, to the small intestines, as well as to the liver? If the pneumogastric nerves control the functions of the liver, they control also fully as much the digestive functions of the stomach and intestines. Digestion, then, would be greatly interrupted in its first processes, and less glucose would be formed from the food, than if the animal were in a state of natural health. Of course, in this case, less glucose would enter the liver through the portal vein. The sugar found in the hepatic veins by M. Bernard was that which has passed through the liver unchanged, as fatty emulsion is sometimes found in the arterial blood immediately after it has passed through the lungs. Ordinarily fatty emulsion, after it leaves the thoracic duct, and enters the circulation through the descending cava—ordinarily, I say—is so changed in its passage from the right to the left side of the heart through the lungs that it can no longer be detected in the blood of the aorta or carotid artery. The same is also, no doubt, the case with the glucose which enters the liver through the portal vein. Sometimes, perhaps, it is all changed, so that no sugar, as such, can be found in the hepatic veins; but when the amount of glucose is large or excessive the liver is not capable of changing it all in its first passage. It is then detected in the hepatic veins.

The livers of lambs, pigs, and beeves, when prepared for the

table, are always sweet. It is because these animals are fattened on food which contains a very large amount of glucose or sugar; not because of any glycogenic function of their livers. The liver is rather a sugar-destroying than a sugar-producing organ. As said above, it separates certain of the coarser elements from the glucose food and forms them into bile, and thus prepares the other part for the more ready transformation into blood. The current of blood through the liver is not like the flow of waters down a river. The same water perhaps never descends the Ohio or Mississippi rivers the second time; certainly not until after it has been evaporated from the ocean and raised to the clouds. As to food, it commences to pass from the stomach and intestines to the liver soon after it is eaten; but when some hours elapse, as proved by the experiments of Dr. Beaumont and others, before all the food has entered the portal veins, then that portion which first passed through the liver has repassed again many times before what is commonly called digestion is completed. The food thus floating in the blood is again and again subjected to the separating and secreting powers of the liver. Physiologists have proved by experiment, certainly to the satisfaction of the most of them, that an increased flow of bile into the duodenum commences with the beginning of digestion, and continues during the whole time of that process. The only reason they give to account for this increased flow of bile is because of the part they suppose it has performed in the transformation of chyle and chyme into albuminose and glucose and fatty emulsion. Strange, astonishingly strange, that they never considered the fact that this bile is secreted from portal blood, at a time when a very large proportion of this blood is made up of fresh glucose and albuminose continually flowing into it.

Glands and the capillary blood-vessels are the only transforming organs in the animal economy. Very certain is it that the food, after being absorbed, is subject to no transforming process before it enters the liver. The constitution of the food is then subjected to its second metamorphosis. It underwent its first change in the stomach and intestines. But the liver has to act

upon the nitrogenized forms of food as well as upon that containing carbon. When the diet is mixed both forms pass through that organ at the same time. What is separated from both is converted into bile. The probability is, for nothing can be determined about it positively, that this variety of bile passes on directly through the hepatic duct and the ductus communis into the duodenum. I suppose so, because of its continued flow into the duodenum while digestion goes on. There is no reason why any part of it should at that time be turned aside into the gall-bladder. Some animals, as the horse, have no gall-bladder. Where they do exist, we must suppose it is to retain the bile in them during a longer or shorter time for some purpose. The food in the blood having undergone its second metamorphosis in the liver, passes on through the hepatic veins into the vena cava, which conveys it to the right side of the heart. From thence it passes to the lungs, where it undergoes its third metamorphosis, being there mixed with the oxygen of the atmosphere. Immediately after this it enters the left side of the heart, which distributes it to all the parts of the body in the general current of arterial circulation. The lymphatics, commencing in all parts of the body where capillary vessels are found, stand ready to separate from the blood a part of this fresh material, which they convey to the thoracic duct. It passes through their glands, and is more highly elaborated, producing, presently, white corpuscles. But a large portion of this food is carried along by the blood to the skin, which, by its perspiratory glands, separates from it a large proportion of its watery contents.

In a former chapter we have stated particularly how large a quantity of fluid is mixed up with the food. We refer to the saliva, the gastric and intestinal juices, the pancreatic juice, the bile, and the lymph. The skin separates a very small fraction of the solid parts of the food, being less, we believe, than five parts in a thousand. But the food is also carried continually along with the current of arterial blood to the kidneys. These separate from it a very large part of its watery elements, and along with this water, urea, creatin, etc., and inorganic sub-

stances. The liver separates from the food substances which it changes into bile. The lungs are continually throwing off carbonic acid and watery vapor. What proportion of the food, solid and liquid, escapes from the lungs in the shape of carbonic acid, or watery vapor, as it—the food—passes through the lungs, I am not prepared to state. The kidneys separate from it a very large quantity of both fluid and solid materials. Physiologists seem to be agreed that there is no catalytic change effected in the kidneys; in other words, that these wonderfully elaborated organs do not create or form urea, creatin, etc. from the blood as it passes through them. They seem to come to this conclusion because urea is found in the blood, as also in the lymph. I suppose that urea is formed from the food as it passes along with the blood through the kidneys, and that this is one of the great processes by which the food is converted into blood. All the materials of the urine, liquid and solid, must have existed at one time in the food and drinks. The urea thus formed in the kidneys does not all sometimes pass off through the ureters into the bladder. A part of it is conveyed by the renal veins into the vena cava. It is thus found sometimes in the blood, and when there is over a certain proportion of it it gives rise to a very serious form of disease. No one denies that the liver secretes bile, but all know that the quantity poured into the duodenum is variable. It no doubt sometimes happens that the urinary ducts fail to convey into the ureters all the urea which had been created. The ureters may sometimes fail in the performance of their duties as conduits to the bladder. When the urea enters the blood through the renal veins, it seems to be one of the functions of the lymphatics to separate it from the blood. Hence urea is sometimes found in the lymph. Why should it be thought strange that the organic and inorganic parts of the urine are separated from the food? Organic life, whether in plants or in animals, consists in the conversion of food and drinks into sap and blood, and the conversion of these latter into organized living tissues or forms. It is in the first part of these wonderful processes of nature that I suppose the separa-

tions to take place. In other words, that the excrementitious matter thrown off from a living body is separated from the food in the process of changing it into blood. Physiologists, perhaps without exception, teach that the food is first formed into living organized tissues; that there is a constructive and destructive metamorphosis continually going on; and that all excretions, properly so-called, except, perhaps, a part of the feces, were once a part of the living body. In this scientific muddle they find it necessary to weigh exactly the quantity of food and drinks swallowed to calculate precisely in how short a time a man's entire body is made out of new material. Dr. Dalton makes the time exactly twenty days. There are cases of typhoid fever in which a man's body is re-created two or three times before his physician pronounces him convalescent. This, of course, is when that wonderfully wise and scientific practice is pursued, as is the rule, according to Dr. Thomas King Chambers, in the London hospitals, where the patients are compelled, willing or unwilling, appetite or no appetite, to swallow every two hours, night and day, a certain quantity of milk and beef tea, amounting in twenty-four hours to a half gallon or more, besides, in many cases, solid food. Such philosophizing has caused physiologists to be unable to understand the functions of the liver. The following extract shows, as fully as it can well be done in so many lines, the absurdities taught by physiologists as to the functions of the liver and kidneys:

"There are two probable sources of urica in the economy, assuming that it always preëxists in the blood, and is not formed in the kidneys. One of these is the disassimilation of the nitrogenized constituents of the tissues, and the other in a transformation in the blood of an excess of the nitrogenized elements of food. Urea, as we have already seen, exists in considerable quantity in the lymph and chyle, and is found also in small proportion in the blood. It has lately been detected in still smaller quantity in the muscular tissue; but chemists have thus far been unable to extract it from any other of the solid tissues, under normal conditions, except the substance of the liver. The fact

that it exists in considerable quantities in the liver has led to the supposition that this is the organ chiefly concerned in its production. With the small amount of positive information that we have upon this point, the view that the liver produces urea, while the kidneys are the organs chiefly concerned in its elimination, must be regarded as purely hypothetical. But if it be true that urea is the result of the physiological wear of the nitrogenized elements of the body, the liver would probably produce its share in the ordinary process of disassimilation. The fact that urea has not been detected in normal muscular tissue is by no means a conclusive argument against its formation in this situation. We have lately shown that, although the liver is constantly producing sugar, none can be detected in its substance, for the reason that it is washed out as fast as it is formed, by the current of blood. In the case of the muscles, it is by no means improbable that the lymph, and perhaps the blood, washes out the urea constantly, and keeps these parts free from its presence during normal conditions. In some late experiments by Meissner, in which the observations of Prevost and Dumas on the accumulation of urea in the blood of nephrotomized animals were confirmed, urea was found in dogs and rabbits, after removal of the kidneys, not only in the liver, but in the muscles and brain. Although our experimental knowledge does not warrant the unreserved conclusion that urea is produced primarily in the nitrogenized parts of the organism, particularly the muscular tissue, this view is exceedingly probable, and we must wait for further information on this subject, until physiological chemists are able to follow out more closely, the exact atomic changes that intervene between the functional operation of organized parts and the change of their substance into excrementitious matter." (Flint's Physiology, vol. 3.)

There is no department of science but medical in which an investigator, who has found out to his entire satisfaction that a particular organ performed one especial function, would jump to the conclusion or conclusions that this organ also performed other and entirely different functions. No physiologist denies

that the special function of the liver is to produce bile. This physiological fact was known to the earliest medical observers. It was for modern philosophers to discover that one of the functions of the liver was to produce, or create, sugar, and that another probable function was to produce, or create, urea. It is not said whether the urea, sometimes discovered in the liver, is found in the lymph or blood of that organ. But this is not all. Physiologists have discovered urea in the muscles, and where the kidneys have been removed, also in the brain; hence they have jumped to the conclusion that both the muscles and the brain-substance may generate urea.

In the animal economy the separation, call it secretion or excretion, of whatever is to be used in the body, or is to be thrown out of the body, is performed by organs called glands. This applies to the saliva, the gastric and pancreatic juices on the one hand, and to the milk, urine, and sweat on the other hand. Bile is sometimes found in the blood, and the whole surface of the body sometimes becomes as yellow as a pumpkin. As well might you say that it was produced in the blood, or created in some inexplicable manner by the skin, as to suppose that urea is formed in the liver, muscles, or the brain, because it is sometimes found in, near, or about those organs or substances. It is only because it is sometimes found in those situations, that microscopic observers have come to the conclusion that it is not formed by the kidneys. Leaving the solid substance in which they say urea is found—liver, muscle, brain—they say also that it is found in larger quantities in the lymph and chyle than in the blood. Here we have six sources for the formation of urea.

Could any other than medical philosophers suppose that Nature thus departs from her universal, invariable law? There is not an instance in the wide domain of Nature in which one definite entity with fixed proportions is produced by, or is the result of, more than one agency or cause. As to the milk, tears, saliva, gastric juice, intestinal juices, pancreatic juice, spermatic fluid, no physiologist even is simple enough to believe or to state that they are formed all over the body. But why do physiologists

deny that the kidneys separate and make from the food urea, creatin, creatinin, and all the salts, etc., found in the urine? It is because they stand upon the baseless hypothesis of constructive and destructive metamorphosis, which they suppose to be continually going on in the animal body. Dr. Flint says, "There are two probable sources of urea in the economy, assuming that it always preëxists in the blood and is not formed in the kidneys. One of these is the disassimilation of nitrogenized constituents of the tissues, and the other is the transformation in the blood of an excess of the nitrogenized elements of food."

If urea is produced by the processes here first stated, the catalytic change must occur in the capillaries, for they are the organs by which all growth and increase are effected on the one hand, and all decrease or waste are effected on the other; in other words, they are the organs of constructive and destructive assimilation and disassimilation. The capillaries empty their contents into the veins. We know that they empty into them a different kind of blood from that which they received from the arteries—blood containing less oxygen and more carbonic acid.

If urea, then, was formed by the disassimilation of nitrogenized living tissues, venous blood ought to be constantly full of it, which is not the fact.

If, however, as I suppose, urea is formed from the food by the kidneys, and if also, as I suppose, all the urea so formed does not constantly pass into the ureters, a part of it escaping through the renal veins into the vena cava, then we can understand how the lymphatics which follow the arteries should take it up from the blood floating in them, thus causing the lymph to contain a greater quantity of urea than the blood, and how it may be found in muscle, brain, or liver-substance.

As to Dr. Flint's second hypothesis, that urea may be formed in the blood itself, it is too absurd to notice.

The fact that urea is found to an excess in animals from which the kidneys had been removed, proves no more as to the functions of the kidneys than do the few instances recorded by anatomists where the ductus communis choledochus does not empty

into the duodenum prove as to the functions of the liver. We know that there are such things as vicarious functions under anomalous conditions. Physiological laws are not to be learned from monstrosities or singular mutilations. We only learn from such the wonderful adaptability of Nature's laws to unusual conditions. To-day, so far as medicine is to be studied as a healing art, there is no gland in the body considered to be so altogether unimportant as to its functions as the human liver. According to the highest medical authorities in Philadelphia, New York, London, and Paris, less attention is paid to the action of the liver in the treatment of diseases than to any other organ in the body. Assimilation and disassimilation, as taught in the books, are constantly before the mind's eye of the medical practitioners. The effete matter so produced is to be removed, according to the same teaching, by the skin and kidneys, especially by the latter organs.

Effete matter is to be considered under two entirely different conditions; the one is effete blood, the other is effete matter which has been a part of the living organism. From the beginning of life in the fetus in utero to the period when growth ceases, provided there has been no sickness and the individual has continued to increase in weight, or not at any time to lose in weight, there is only effete blood. During this period, let it be longer or shorter, all the excretions proper, all that which is thrown off from the body, is the result of the conversion of the food into blood. This does not apply, of course, to the trimming of the nails and cutting of the hair, nor to the shedding—so to call it—of the cuticle or scarf skin, or the removal of the epithelial covering of the mucous passages; nor does it apply to the teeth. Neither of these are excretions in the common acceptance of the word. Crawfish, crabs, and lobsters shed their shells or outer coverings every year. Deer and elk shed their horns every year. If the law of constructive and destructive metamorphosis were true, as taught by physiologists, neither the crustacea would shed their shells nor would deer and elk shed their horns. According to this law both the shells and horns would

have been renewed eighteen times in a year with five days to spare, allowing twenty days for the complete metamorphosis, according to Dr. Dalton. The shells of crabs and lobsters and the horns of deer and elk are as hard as bone.

It is a singular fact about deer, that if they are castrated after their horns are fully grown, that is, after having passed out of the velvet state and become hard, they never shed them again. I have been told that when they are castrated without horns the latter never grow out again. The same law may apply to elk. It is impossible for physiologists to explain these facts and still maintain the doctrine they teach. But it is not a matter of little or no consequence whether they or I teach the truth. The laws of hygiene, the diet of the sick, and the treatment of diseases, are all made to depend upon what is or what is not effete matter.

In 1838 Mr. W. Grisenthwaite published in London an *Essay on Food*. He denies this doctrine of metamorphosis or total change of the body in so many weeks, months, or years. He goes into very nice and particular calculations to show that in a healthy man, not increasing in weight, the amount of the excretions correspond exactly with the amount of food and drink swallowed. I do not pretend to say whether his calculations are correct or not. I know that there are many erroneous positions taken in his book. Besides the general fact, I agree with very little that he says. He promised to explain several questions more fully, but this little essay is the only one of his works I ever saw or heard of.

To return, I maintain that the first form of effete matter is effete blood, blood which has served its purposes but was never a part of living organized forms. This effete blood was eliminated from the body by the lungs and liver, in the one case it being thrown off as carbonic acid, in the other as bile. This is the only elimination in a healthy body after the food has been formed into blood by the various and wonderful processes through which I have followed it in these pages.

It is not within the scope of this chapter to speak of the carbonic acid thrown off by the lungs in a state of health; I will

only state the fact that the lungs throw off much larger quantities of this effete blood in the form of carbonic acid than does the liver in the form of bile. The quantity of carbonic acid thrown off by the lungs in a healthy man weighing one hundred and forty pounds has been estimated as high as eleven ounces in twenty-four hours. The throwing off of carbonic acid by the lungs is not more important in the maintenance of health than is the separation of bile by the liver. Indeed, it can hardly be considered as much so, for the carbonic acid goes immediately out of the body, whereas the effete blood separated by the liver is converted into bile, which has important functions to perform. During fetal life the liver is the only gland by which effete *blood* can be removed from the living system. Until the child breathes no carbonic acid can be thrown off, nor urine from the bladder. If the metamorphosis doctrine of physiologists were true, and if, also, the kidneys separated effete matter from the blood, then the bladder ought to be enormously distended; indeed it ought to be many times larger than the body of the new-born infant; for, dividing two hundred and seventy days, the period of fetal life, by twenty days, the time of entire renewal, according to Dr. Dalton, the bladder would contain the debris of thirteen and a half fetal existences. Such are the absurdities to which this doctrine leads us. Physiologists deny that the meconium found in the intestines of new-born infants comes from the liver, because bile can not be detected in it, according to Pettinkofer's test, although they admit that traces of bile are found in the small intestines as early as the fifth or sixth month of fetal life. The constituent elements of saliva, of the gastric and pancreatic juices, and of mucus, are well known. I do not suppose that any physiologist will maintain that either saliva, gastric, or pancreatic juices are poured out into the duodenum during fetal life. The only sources, therefore, from which substances found in the intestines at birth can come, are the bile and mucus. It is a mixture of both.

According to Dr. Flint, one of the methods by which urea may be formed is that it is generated in the blood-vessels. It is a ridiculous absurdity, but he and those who agree with him

must admit that it would not be as singular catalytic change to form meconium out of bile and mucus and epithelial cells, if you choose, as it is to form urea *de novo* out of pure blood. If you can not detect bile in meconium, you can not detect blood in urea. The quantity of meconium found in the bowels of newborn infants varies considerably. It no doubt depends greatly upon the healthy or sickly life of the fetus. The size of the liver is, during that period and for some time after birth, much larger in proportion to the weight of the body than at any other time during life. Effete blood is the only substance to be removed from the circulatory system of uterine fetal life, and it is the especial duty of the liver to remove it. No other secretory excretion then takes place. According to my observation during a practice of forty-three years, and according to that of many physicians far more distinguished than I am, running back for generations, there is not in the whole materia medica a single medicine which acts so kindly and curatively as calomel in the treatment of the affections of infants and young children. The distinguished Dr. Benjamin W. Dudley used to say to his class that an old Scotch doctor was wont to say that with the blessing of God and a phial of calomel he could cure all the children in the kingdom, and Dr. Dudley concurred with the Scotch physician.

The second form of effete matter comes from the waste of the solid parts of the body—call it destructive metamorphosis or disassimilation, as you please. It is never separated from the solid parts except with a corresponding loss of weight of the whole body. It may be so separated during good health by exercise or labor, as particularly in the training of a race-horse or an athlete; but in the human race it far more frequently occurs when the organic life of the individual is interfered with by some sort of sickness. As to this form of effete matter, we do not have to go dodging and hunting around with a rush-light and microscope to see whether it is produced in the liver, in the muscles, or in the brain, or whether it is found in the lymph, in the chyle, or in the blood. We know that it is separated from

the living organism in every part of the body by the capillary blood-vessels. I say that we know that such is its origin, because no physiologist disputes the fact that capillary vessels, call them by what name you please, do separate milk, saliva, gastric juice, pancreatic juice, bile, etc., each of which is made up of both solid and liquid constituents. That this separation of effete matter takes place in some part of the organism to a far greater extent than in other parts, there can be no doubt.

Perhaps there is less effete matter taken from the substance of the nerves, the bones, and the brain, than from other parts of the body. This is a matter which deserves investigation by physiologists.

The question is, how is this form of effete matter removed from the body? I answer that it is removed principally by the lungs and by the liver. But it may be asked, do the kidneys remove none of it? A very large percentage of all forms of living matter, whether vegetable or animal, is water. It is the special functions of the skin and kidneys to remove water from the body, and they carry off this water, whether the body is reduced in weight by sickness or starvation as well as in health. But it is a well known fact to all physicians of experience that, except a few forms of disease, as diabetes and some nervous affections, the quantity of urine eliminated is much less in sickness than in health.

In all pathological states or conditions, where little or no food is taken, and where effete matter is being removed from the solid organism, the kidneys may, and no doubt do, separate organic substances from the blood, and form or create such substances as urea, etc. out of the effete matter floating in the blood. It seems to be the special functions of the kidneys to separate all foreign substances from the blood. We know how rapidly they separate bromide of potash from the blood. Effete matter, let it be formed in whatever way you please, is a foreign substance floating in the blood.

We speak of men and women being in good health, while there are very few who are not subject to some irregularity which dis-

turbs more or less the proper performance of the functions of vegetative or organic life. In all of these instances effete blood or effete solid matter may exist. A man may weigh a few ounces less to-day than he did yesterday. The effete matter then formed is in proportion to the weight lost. In these instances the kidneys may remove matter, other than that taken from the food, from the blood. The foreign substances to be removed by the kidneys, as the bromide of potash, are things swallowed and mixed up with the food.

There is nothing more variable than the constituents of the urine. Sometimes mental emotions, even, will change the quantity of urine and the elements of which it is composed. The kidneys are capable of separating many things from the blood. They separate bile from it when the bile is not poured out into the duodenum after being secreted by the liver; but while they are yet capable of separating foreign substances, they seem to be dependent upon the liver for the proper performance of their functions. You may sometimes force them to act by giving diuretics, but such medicines do not effect any curative results in the treatment of disease unless the liver is performing its functions properly at the same time; and, according to my medical experience of forty-three years, there is very seldom any necessity for giving diuretics if you see to it beforehand that the liver is secreting the proper quantity of bile. I could give numerous cases treated by other physicians in Kentucky where patients who had taken many forms of diuretic medicine for some supposed affection of the kidneys were promptly cured by taking medicine which acted upon the liver—as calomel; calomel and ipecac; rhubarb, ipecac, and calomel. When the liver does not perform its functions healthfully, the constituents of the urine are never such as are found in a person in good health. When the kidneys do not separate effete matter, or any other element from the food, which they ought to separate from it, if you will make the liver secrete bile more freely, you will soon see in all critical cases that the kidneys are again able to perform their proper functions. The all-important difference be-

tween the offices of the liver and kidneys is this, that so soon as the urine is poured into the bladder it is of no further use, and is regularly discharged from the body. On the other hand, the *secretion* of the liver—the bile—has several important functions to perform. These we will now consider.

- No man ever saw one drop of pure blood. It is impossible for the analytic chemist to discover its constituent parts, for he can not obtain a single drop for his analysis.

No man ever saw a drop of pure water, except a chemist and those who have seen the results of the chemist's analysis. By distillation all organic and inorganic substances floating in water can be separated from it, and we have the thing called water composed of oxygen and hydrogen in definite proportions; yet we call it water, whether taken from the ocean, Saratoga, White Sulphur, or Blue Lick Springs. So it is with the blood.

Physiologists have discovered that food is absorbed under three forms—glucose, albuminose, and fatty emulsion. Whatever else may be absorbed along with either one of these forms they do not know, for they are all immediately mixed with the blood, which is composed of many elements. In the absorption of water, spirits, wine, malt liquors, tea, and coffee, I do not think it has yet been determined whether the absorbent veins reject any part of them or swallow them all. These liquids are taken up together with the food. The food and drinks can not be said to be converted into blood until they have passed through the portal vein, the liver, the hepatic veins, and the ascending vena cava on the one hand, and the lacteals, the mesenteric glands, and the thoracic duct on the other; and then both through the right side of the heart, the pulmonary artery, and lungs into the left side of the heart through the pulmonary veins. I say that the food and drinks can not be called blood at all until they have passed through the stomach and intestines, through the glands and vessels referred to, to the aorta. You can not obtain blood, then, even approximately pure, in the portal vein, in the hepatic vein, in the vena cava, or even in the pulmonary artery. But this is not all: glucose and fatty emul-

sion sometimes pass this round and are found in the arterial blood. No physiologist disputes this fact. In this round referred to, the liver and the lungs effect certain changes in the food, but after it has entered the arterial circulation it has to be subjected to other eliminating processes which separate from it both organic and inorganic substances. The special work of the kidneys is to perform this latter work.

I might refer also again to the functions of the lymphatics, the spleen, and the skin, but it is unnecessary. This is the state of things in what we call good health. It is an endless chain. Food and drinks are being continually poured into the current of circulation. The conditions under which they enter this circulation is a variable one. Such is the blood in a healthy man. But let there be wasting sickness, then we have the current of circulation overcharged with effete matter, and also, it may be, with blood-poison, as that of smallpox, or scarlet fever, or miasmatic malaria. Or the food may be unwholesome and badly prepared by a dyspeptic stomach. Be it as it may, it is this mixed fluid floating in the veins and arteries, from which the liver separates bile. But since physiologists have demonstrated, as they suppose, that the food becomes blood as soon as it is absorbed from the stomach and intestines; that what was beef-steak at breakfast yesterday has become brain and muscle to-day, and will appear again to-morrow in the form of effete matter, and that this effete matter is chiefly urea, is it to be wondered at that they suppose the kidneys to be the most important glands in the body for the removal of effete matter? "The urine is an excretion, its constituent parts are excretions. These constituent parts are derived from the metamorphosis of the solid organism. These latter are effete matter." Such are the teachings of physiologists. Therefore, say they, the kidneys are the most important excretory glands in the body. They say a man can only live so many hours, or a few days at most, without urinating, but that he can live for many weeks without having a passage from his bowels. Therefore, say they, whatever is secreted or excreted by the liver is of very little consequence. They

neither know what the bile is made of, nor what are its uses after it is made. I deny that in perfect health the kidneys separate effete matter at all from the blood. If they do separate any in disease it is under particular circumstances; which I will presently explain.

Effete matter is separated from the solid organism by the capillaries. That system of vessels is intermediate between the terminations of the arteries and the beginnings of the veins. In all of the organs within the abdominal cavity of a man, except the kidneys and urinary apparatus, the capillaries empty the effete matter, which they have separated, into the veins, which carry it directly to the liver. This much of effete matter is subjected to the action of the liver as it passes through it; it then passes on through the hepatic vein and vena cava to the lungs, which glands again act upon it. The effete matter emptied into the veins by the capillaries, in every other part of the body, is returned to the right side of the heart by the ascending and descending vena cava, and it, too, has to pass through the lungs. In other words, all effete matter has to pass—a part of it through the liver, and all of it through the lungs, before it enters the arterial blood. In a healthy condition of the body, these two glands separate the whole of it from the blood. If they fail to perform their proper offices, and the liver is frequently unable to do its work, then some effete matter may enter the left side of the heart, and be carried from thence to the kidneys.

The lymphatics were, for a long time, supposed to be the only absorbents. It is now known that they separate from the blood, or rather from the food floating in the blood, as I believe, highly vitalized elements, as fibrin, albumen, and white corpuscles. I think they form them. But if the lymphatics separate effete matter at all from the blood, we know that they, too, empty their contents into the ascending and descending vena cava, and thus the lymph, too, has to pass through the lungs before it enters the left side of the heart. Urea is found in the lymph. It is a nitrogenized substance. The lungs throw off by expiration a certain amount of nitrogen. In what particular form it throws

it off, I am not prepared to say. The lungs may throw off urea. Whether or not they are able to separate urea into its elements I do not know. We all know that the breath from many persons is terribly offensive. The lungs certainly separate from the blood, sometimes, other substances beside carbonic acid and water. The breath of men who are accustomed to smoke tobacco continually, to drink whisky in large quantities, and to eat onions as they would cabbage or potatoes, certainly contains something beside carbonic acid and water.

The purest arterial blood in the body goes to the kidneys, and the most vitiated blood goes to the liver. The liver is at all times separating from the blood what ought to be removed. That is its duty, but the liver does not always do its duty. When it fails to do its proper work, indisposition, or serious illness is the consequence. In that state of the case, its near neighbors—the kidneys—are unable to do their proper work. Indeed, then, every other organic, vital, animal, and mental power is weakened. What the liver separates from the blood, it forms into bile. I have already given the composition of the human bile in Chapter I.

We now copy from Dr. Flint:

“Secretion of Bile from Venous or Arterial Blood.—Numerous experiments have been made with the view of determining whether the bile be secreted from the blood brought to the liver by the portal vein or from the blood of the hepatic artery. The immense quantity of blood distributed in the liver by the portal vein led first to the opinion that the impurities were separated from this blood to form the bile, and that the hepatic artery had little or nothing to do with the secretion. This, indeed, was the view adopted by Glisson, one of the earliest writers on the anatomy and functions of the liver. But since Bernard discovered the glycogenic function of the liver, this subject has assumed additional importance, and it becomes a question whether the materials for the secretion of bile may not be furnished by one vessel (the hepatic artery) while the other (the portal vein) is specially concerned in the formation of glycogenic matter. This theoret-

ical view, however, is not carried out by well-established anatomical facts or by physiological experiments. It is not possible to separate the liver anatomically into two organs, one for the secretion of bile, and one for the production of sugar. It seems certain also, from numerous experiments, that bile may be secreted from the blood of the portal vein after a ligature has been applied to the hepatic artery, and it is equally certain, from the recent experiments of Oré, that if the portal vein be obliterated so gradually that the animal does not die from the operation, bile is secreted from the blood of the hepatic artery."

"The experiments of M. Oré are very curious and instructive. After having repeatedly made the experiments of applying a tight ligature to the portal vein, producing thereby very grave symptoms and death so speedily that the effects upon the secretion of bile could not be satisfactorily observed, he modified his operations so as to effect a gradual obliteration of the vein. This he accomplished by simply applying a loose ligature, and tightening it from time to time till it came away. By this mode of procedure he succeeded in observing the secretion of bile in six days or more after the application of the ligature; and on killing the animals he found the portal vein entirely obliterated and no communicating branches by which the blood could get from the portal system to the liver. From these observations it is concluded that the bile is secreted from the blood of the hepatic artery."

"In support of this view, several instances of obliteration of the portal vein in the human subject are cited in works upon physiology. In a note to the communication of Oré, in the *comptes rendus*, Andral reports the case of a patient that died of dropsy, and on post-mortem examination the portal vein was found obliterated. In this instance the gall-bladder was found full of bile. In addition, instances in which the portal vein emptied into the vena cava have been reported, and in none was there any deficiency in the secretion of bile."

"In the experiments upon the effects of tying the hepatic artery and the observations of instances of obliteration of the

portal vein and of congenital malformation in which the portal vein does not go to the liver, be equally reliable, there is but one conclusion to be drawn from them, and that is that bile may be secreted from either venous or arterial blood. This view is not inconsistent with what we know of the general process of secretion and its application to the production of bile. Regarding the bile as in part an excrementitious fluid, its effete element—cholesterin—is contained both in the blood of the portal vein and in the hepatic artery. Its re-crementitious principles—glycocholates, taurocholates, etc.—we suppose are produced *de novo* in the liver out of materials furnished by the blood. The exact nature of the production of elements of secretion by glandular cells we do not understand; but there is no good reason to suppose that the principles necessary for the formation of bile may not be furnished by the blood of the portal vein, as well as by the hepatic artery.”

“The views most in accordance with all the facts bearing on the question is that the bile is produced in the liver by the blood distributed in its substance by the portal vein and the hepatic artery, and not from either of these vessels exclusively, and that the bile may continue to be secreted, if either one of these vessels be obliterated, provided the supply of blood be sufficient.” (Flint, vol. 3, pages 253–255.)

Dr. Frerichs, in his elaborate work on The Diseases of the Liver, says, “With regard to no organ does history attest a change of views in a more remarkable manner than in the case of the liver. This organ and the portal venous system attracted at a very early period the attention of physicians. More from vague conjectures than from clearly-established grounds, the seat of various functions of great importance, both in health and disease, was transferred to this powerful glandular organ, and to the extensive system of vessels which is intimately connected with the gastro-intestinal canal. By the ancients the liver was regarded as the central organ of vegetative life. Galen looked upon it as the focus of animal heat, and as the organ intended for the formation of blood, and for the origin of the veins. According to him

the metamorphosis of chyle into blood commenced in the portal veins, but was completed in the liver, which organ during the process, separated as waste matter from the blood the yellow and black bile, the former passing to the gall-bladder, and the latter to the spleen.

Galen's views underwent scarcely any modifications by the Arabian physicians, and remained undisputed until the middle of the seventeenth century. Even Vesalius, who, by his anatomical researches, contributed more than any one else to the overthrow of the doctrines of Galen, did not venture to oppose his physiological theories respecting the liver; he only denied the assimilative property attributed to the portal veins.

The attempts of Argentieri to restrict the functional importance of the organ met with no support. The discovery of the lacteal vessels by Aselli in 1622, and of the thoracic duct by Prequet in 1647, first imparted a severe shock to the views of Galen. A way had been discovered by which the chyle was conveyed from the bowel into the blood quite independently of the portal vein and of the liver; henceforth neither of these structures appeared to serve any purpose in sanguification. Bartholin and Glisson were the first who enunciated in a decided manner this opinion. It rapidly spread; and the more readily, inasmuch as the revolutions just effected in the views of physiologists by the labors of Harvey, bestowed an importance hitherto unknown upon the organs of the chest. Riolan, indeed, and after him De Bils endeavored to defend the important part played by the liver in sanguification in opposition to the views of Bartholin; but Bartholin came victoriously out of the contest, and wrote for the liver a humorous epitaph, in which the end of its dominion was announced, and in which its function was declared to be henceforth limited to the secretion of bile.

For almost two centuries this opinion continued generally to prevail. Swammerdam, it is true, endeavored to advocate once for the ancient theory, yet with so little success that Boerhaave observed, "*Dudum in meliori parte Europæ abolevit hæc sanguificatio nunquam ab eo viscere expectanda.*" It was reserved for the

experimental physiology of the present century to extend the boundaries of our knowledge in this matter, and to reproduce in a more novel and exact form opinions which, for a long period, had appeared buried in oblivion. The first step in this direction was made by Magendie and Tiedemann, who furnished the proof that the absorption of nutritive matter is not solely the work of the lacteal vessels, but that a part of the food digested in the gastro-intestinal canal is taken into the blood through the portal vein. Tiedemann and Gmelin moreover, by a series of careful experiments, arrived at the result, that "the liver must also be regarded as the organ for the assimilation of substances which have been absorbed from the intestines."

The more recent investigators who have paid attention to this subject, such as Blondlot, Claude Bernard, Lehmann, C. Schmidt, and others, have been obliged to corroborate the opinion as to the participation of the portal vein in the absorption of materials from the intestines into the system, although much remains doubtful as regards the extent and importance of this participation. It is to be regarded as clearly proved that the water, salts, sugar, odoriferous and coloring matters are, for the most part, conveyed to the blood by venous absorption, as also that the greatest part of the fat reaches the blood through the lacteals. But it is still a matter of question in what way the most important principles of nutrition—the albuminous—are absorbed. Claude Bernard, and the majority of the French investigators, make their absorption take place through the veins, while Lehmann, Schmidt, and Ludwig are convinced that it is through the lacteals.

Another point which has not been sufficiently investigated is the influence which the hepatic parenchyma exercises over the substances which pass through it. According to the experiments of M. Bernard, Mealhe, and others, the carbo-hydrogens, no less than the albuminous principles, in their passage through the portal system, undergo important changes from the action of the liver, by means of which they first become fitted for the formation of blood. A detailed proof of this metamorphosis has not, however, as yet, been given with sufficient clearness.

That great thinker, Thos. Henry Buckle, in his *History of Civilization*, says, "As these views have a social and economical importance quite independent of their physiological value, I will endeavor in this note to fortify them still further by showing that the connection between carbonized food and the respiratory functions may be illustrated by a survey of the animal kingdom."

"The gland most universal among the different classes of animals is the liver; and its principle business is to relieve the system of its superfluous carbon, which it accomplishes by secreting bile, a highly carbonized fluid. Now the connection between this process and the respiratory function is highly curious, for, if we take a general view of animal life, we shall find that the liver and lungs are nearly always compensatory; that is to say, when one organ is small or inert the other is large and active. Thus reptiles have feeble lungs, but a considerable liver; and thus, too, in fishes, which have no lungs, in the ordinary sense of the word, the size of the liver is often enormous. On the other hand, insects have a large and complicated system of air-tubes, but their liver is minute, and its functions are habitually sluggish. If, instead of comparing the different classes of animals, we compare the different stages through which the same animal passes, we shall find further confirmation of this wide and striking principle. For the law holds good, even before birth; since in the unborn infant the lungs have scarcely any activity, but there is an immense liver, which is full of energy, and pours out bile in profusion. And, so variable is this relation, that in man the liver is the first organ which is formed. It is preponderant during the whole period of fetal life, but it rapidly diminishes, when, after birth, the lungs come into play, and a new scheme of compensation is established in the system."

These facts, interesting to the philosophical physiologist, are of great moment in reference to the doctrines advocated in this chapter. Inasmuch as the liver and lungs are compensatory in the history of their organization, it is highly probable that they are also compensatory in the functions they perform, and that what is left undone by one will have to be accomplished by the

other. The liver, therefore, fulfilling the duty, as chemistry teaches us, of decarbonizing the system, by secreting a carbonized fluid, we should expect, even in the absence of other evidence, that the lungs would be likewise decarbonizing; in other words, we should expect that if, from any cause, we are surcharged with carbon, the lungs must assist in remedying the evil. This brings us, by another road, to the conclusion that highly carbonized food has a tendency to tax the lungs, so that the connection between a carbonized diet and the respiratory functions, instead of being, as some assert, a crude hypothesis, is an eminently scientific theory, and is corroborated, not only by chemistry, but by the general scheme of the animal kingdom, and even by the observation of embryological phenomena.

The views of Liebig, and of his followers, are indeed supported by so many analogies, and harmonize so well with other parts of our knowledge, that nothing but a perverse hatred of generalization, or an incapacity for dealing with large speculative truths, can explain the hostility directed against conclusions which have been gradually forcing themselves upon us since Lavoisier, seventy years ago, attempted to explain the respiratory functions by subjecting them to the laws of chemical combination.

“In this and previous notes I have considered the connection between food, respiration, and animal heat at a length which will appear tedious to readers uninterested in physiological pursuits; but the investigation has become necessary on account of the difficulties raised by experimenters, who, not having studied the subject comprehensively, object to certain parts of it. To mention what, from the ability and reputation of the author, is a conspicuous instance of this, Sir Benj. Brodie has recently published a volume (*Physiological Researches*, 1851) containing some ingeniously contrived experiments on dogs and rabbits (eighty years ago Bichat taught the same doctrines as those expressed by Buckle in the last few sentences) to prove that heat is generated rather by the nervous system than by the respiratory organs. Without following this eminent surgeon into all his details, I may be permitted to observe, that as a mere matter of history,

no great physiological truth has ever yet been discovered by such limited experiments on a single class of animals; and this is partly because in physiology a crucial instance is impracticable, owing to the fact that we deal with resisting and living bodies, and partly because every experiment produces an abnormal condition, and this lets in fresh causes, the operation of which is incalculable."

I have already referred to the glycogenic function of the liver. It is repeatedly said by those who pretend to have some knowledge of physiology, and who claim to be up with the times, that the glycogenic functions of the liver have been fully established by M. Claude Bernard, and it is asserted to be a crucial test of the truth of this discovery, that if the floor of the fourth ventricle of the brain be irritated, the glyeogenic function of the liver is excited or increased.

This is an age of music without melody, and of science without thought. If it be a fact, which I doubt, that the irritation of the floor of the fourth ventricle of the brain causes a larger amount of sugar to be found in or about the liver, it is a proof of my theory and not of M. Bernard's. Vivisection or torture never excited or increased the healthful function of any organ. I have already referred to the fact that a large amount of glucose is continually passing, during digestion, from the portal vein into the liver; and I have affirmed that one of the offices of the liver is to convert this glucose into something more akin to blood. The torture, then, inflicted upon the floor of the fourth ventricle of the brain, would simply arrest the faculty of the liver to convert the glucose into something nearer akin to blood, and hence more sugar would be discovered in the hepatic veins.

As Virgil says in his first Eclogue, on his journey from Padua to Rome, "If we may compare small things with great ones," I would say, that the difference between Lavoisier and Liebig, as to the production of animal heat, and the theory which I teach is this, viz. that animal heat is not produced in any one organ, but that it is generated by glandular action in every part of the body. The blood found in the hepatic veins is

hotter than in any other part of the body, because there are six systems of circulation going on in the liver, to wit, arterial, two venous, portal and hepatic veins, lymphatics, biliary ducts, and nerves. The lungs may approximate the liver in the production of animal heat, but a large amount of it escapes as latent heat in the watery vapor expired from the lungs.

I wish the reader to refer to the table of the constituents of the bile in a former chapter. I think I have shown to the satisfaction of every one, who allows himself to think, that the supposed discovery of the glycogenic function of the liver by M. Bernard is one of the results of microscopic examination, to the exclusion of general laws. I believe that physiologists will yet come to agree with Bartholin that the function of the liver is limited to the secretion of bile. But according to my theory this bile is, in health, for the most part, secreted from the food as it passes through the liver; and in that sense the liver is an organ actively engaged in sanguification. Fatty emulsion is sometimes found in the hepatic veins.

Frerichs, vol. 1, page 196, says, "Deposits of fat in the tissue of the liver are amongst the most frequent structural changes observed in the organ. When this deposit attains a high degree, we are wont to regard it as a disease, and to designate it by the name of fatty liver, or fatty degeneration of the liver. All attempts to sketch an accurate history of this anatomical lesion from clinical observations have proved unsuccessful; fatty liver is met with so frequently on opening the dead body, and all clues to diagnosis during life are so inaccessible, that it is impossible to construct a satisfactory symptomatology of the affection."

I am not considering pathological alterations in the liver or any other organ; but when fatty emulsion even is found in the blood of the hepatic veins, I consider it to be an irregularity, and not the normal state of a man in perfect health.

According to Dr. Flint, up to the time of the discovery of M. Bernard, it was not doubted that bile was secreted from the portal blood. Since that discovery it is maintained by physiologists that bile may be secreted from the blood brought to the

liver by both the portal vein and the hepatic artery, or by the hepatic artery alone. Some remarkable facts are given to support this theory.

We are told that the portal vein has been obliterated, and that the blood which it carried could not by any possibility reach the liver, and yet the animal lived. The dogs experimented on are said to have lived six days or more. It is not stated upon what kind of food they were fed.

"Communications exist between the portal vein and vena cava which, when enlarged, convey a large portion of the portal blood direct to the heart without passing through the liver. In the first place, there is the anastomosis of the internal hemorrhoidal with the inferior hemorrhoidal veins, which pass into the hypogastric vein, and likewise the anastomosis of the left coronary with the esophageal and diaphragmatic veins. More rarely we find anastomoses of the hemorrhoidal with the vesical veins, of the coronary vein of the stomach and of the gastro-epiploic veins with the renal vein, of the superior mesenteric vein with the left renal vein, and of the vasa brevia with the left phrenic. There are few direct proofs, however, that these vessels become enlarged in cirrhosis." (Frerichs, vol. 2, page 81.)

If the livers of the dogs whose portal vein had been thus occluded secreted bile, we can see from the extract just given that a portion of the blood in the portal vein could reach the general circulation; indeed it must have done so, or the animals would have died in a much shorter time. But a case or cases are reported where the portal vein emptied into the vena cava, and yet the liver secreted bile. Therefore, say physiologists, bile may be secreted from arterial blood. In such a case the hepatic artery is the only blood-vessel conveying blood to the liver.

In the case referred to by Mr. Abernethy, in which the venæ portæ did not enter the liver, but terminated in the inferior vena cava, it was found that the hepatic artery was one third larger than usual, evidently suggesting the idea that it was in the performance of some unusual office. Besides, the umbilical vein was still pervious, and branched out into the substance of the

liver. In this case the blood would be considerably mixed. It could not be called arterial blood at all. The liver would, no doubt, take a much longer time to separate from it a given quantity of bile. The blood conveyed into the vena cava by the portal vein, in this case, would be carried to the lungs. We ought to have been told something about the excretions from the lungs in the case of this remarkable man. Perhaps it would have been observed that the lungs are capable of separating many substances from the blood besides carbonic acid and water.

Dr. Flint says, I repeat, "Regarding the bile as in part an excrementitious fluid, its effete element—cholesterin—is contained both in the blood of the portal vein and in the hepatic artery. Its recrementitious principles—glycocholates, taurocholates, etc.—we suppose are produced *de novo* in the liver out of materials furnished by the blood." I do not believe that cholesterin ever was found in the blood of the portal vein or hepatic artery in a perfectly healthy person. Cholesterin is manufactured out of the blood by the liver, and by no other organ, just as urea is manufactured out of the blood by the kidneys, and by no other organs. If either is found in the blood it is because they have passed through the glands which secreted or excreted them, call it which you please.

When the biliary ducts fail to empty themselves into the hepatic duct on the one hand, or the urinary ducts into the uretery on the other, then, and only then, both cholesterin and urea may find their way into the general circulation. But it will be said that when the kidneys have been removed from the animals an excess of urea is found in the blood. I will be asked, how is that formed? I have repeatedly said that urea and all other excrementitious substances found in the urine of a healthy person are separated from the food floating in the blood by the kidneys; that it is one of the most important processes by which the elements of the food are converted into blood proper. In disease I admit that they may and do separate other substances from the current of the circulation. When the kidneys have been removed the glands remaining to act upon the food floating in the

blood—upon effete blood, or effete matter proper—are first the liver, the mesenteric glands, and the lungs. These act first upon the food and chyle which have been mixed up with the venous blood. After the food and the chyle have passed through these organs they enter the arterial circulation, and are then additionally subjected to the action of the spleen and lymphatic glands. We know that vicarious functions are performed in the animal economy. It is one of the methods by which nature relieves herself of difficulties. The time may come when some experimenter will be able to remove the liver from animals. We know that the brain has been removed from pigeons, and yet they lived for some days. If the liver were carefully sliced away so as to allow the blood still to flow from the portal vein to the hepatic veins or to the vena cava, I do not think it would be as fatal to life as the obliteration of the portal vein. Should this experiment ever be successfully performed, it will, perhaps, be discovered that some other organ is capable of separating bile from the blood. Must we, then, conclude that the liver is not the gland which secretes bile? We are told that cholesterin is an excrementitious principle or substance, and the glycocholates, taurocholates, etc., are excrementitious principles or substances. We will notice this presently.

Dr. Flint says, "The exact nature of the production of elements of secretion by glandular cells we do not understand." This may be true, but if physiologists know any thing, they certainly know this, that secretion or excretion, call it which you please, is only effected by glands. No other organs secrete.

It would be impossible to conjecture how physiologists can imagine that the nerves or muscles can secrete such a substance as urea or cholesterin, did we not know the falsity of the facts upon which they base their conclusions. I know that Dr. Flint says, "In the nervous substance and in the crystalline lens, it is united '*molecule á molecule*' to the other elements which go to make up those tissues." I can not conceive that he means that an excrementitious substance or principle can be a constituent part of a living, healthful tissue; nor can I suppose that the

capillary vessels in separating effete matter from the living tissues do so separate it in the form of organized substances, as cholesterin and urea. Cholesterin is found in the meconium of a new-born infant. Where does it come from? Those who would deny the functions of the liver, pretend to say that meconium is made up of epithelial cells and mucus. They can not stultify themselves by maintaining that cholesterin, a crystallizable substance, is made out of epithelial cells and mucus. Nothing, except, it may be, a little mucus, can pass from the stomach into the duodenum of the fetus in utero. There is but one other passage opening into that gut, the ductus communis choledocus. The existence of cholesterin in the meconium proves two things; 1. That it, cholesterin, is produced in the liver; 2. That it and meconium both come from the liver.

The amount of bile said to be produced daily in the body of a man weighing one hundred and forty pounds, is estimated at two pounds and a half. Except cholesterin, a very small constituent part of the bile, all the rest is said by physiologists to be recrementitious. None of its constituent parts, say they, can be found in the feces. Physiologists admit that they do not know what becomes of it. But Dr. Flint says that, whether changed or unchanged, it is certainly reabsorbed. He ought to have omitted the adverb certainly in making such an assertion. He, and all other physiologists admit that the constituents of the bile glycocholate, taurocholate of soda, etc., can not be found as such in the blood of the portal vein. If they are absorbed they have nowhere else to go to but back into the portal vein. What a wonderfully exceptional contradiction in the workings of the animal economy, for two pounds and a half of a singularly compounded fluid to be poured into an open gut, and that fluid to be instantly afterward reabsorbed and taken back to the liver, where the same work is to be performed over again, and this, too, for a man's whole lifetime! And this is called science! I think it was Dr. Radcliffe, when asked by a young man to tell him what books to read to become a doctor, who said, "Read Don Quixote."

On the origin of bile of jaundice Dr. Frerichs says, "Copious materials have accumulated in the course of time, and theoretical views of various natures have started up and fallen to the ground without the most important fundamental questions having received a satisfactory explanation." "In more recent times this theory has acquired a still firmer foundation, since the chemical and physical properties of the bile have begun to be examined more carefully, and since the constituent elements of this secretion have been sought for in vain in the blood of the portal vein, and likewise of other vessels."

"Zenker and Funke have brought forward fresh arguments in favor of the intimate relation subsisting between the bile-pigment and the red matter of the blood, by showing that a modification of the coloring-matter of the bile, viz. bilifulvine, can easily be transformed into hematoidin, a derivative of 'hematin.' This would appear to indicate the possibility of a direct transformation of hematin into cholepyrrhin."

"All the means for detecting traces of the essential elements of the bile in the blood generally, and in that of the portal vein in particular, have been exhausted without any result; neither the coloring-matter, nor the acids of the bile, substances for which we possess tests of considerable delicacy, have been found. Even if it be granted that the quantity of bile which circulates with the blood under normal circumstances is immeasurably small, owing to the constant performance of the functions of the liver—in the same way as occurs in the case of the urine—still this argument is of no weight in diseased conditions, in which the disorganization of the glandular parenchyma limits the secreting function, or arrests it to a greater or less degree. In the same way that urea accumulates in large quantity in the blood in granular degeneration of the kidneys, so ought the biliary acids and bile-pigment to accumulate in the blood in cases of granular liver. Repeated observations have proved that this is not the case."

"Still more decided are the proofs of the formation of bile in the liver, which have been furnished by the beautiful investi-

gations of Müller and Kunde, and likewise of Moleschott. In the case of the frogs which were kept alive for two or three days, and by Moleschott for several weeks, after being deprived of their livers, not a trace of the elements of the bile could be detected in the blood, the lymph, the urine, or the muscular tissue. The idea of the preëxistence of bile in the blood is further opposed by the arguments in favor of conversion of certain elements of the blood into the bile within the liver, which may be derived from the different composition of the blood in the portal and hepatic veins. Although the difference of the blood from these two sources indicated by Lehmann's analyses appears almost too great to be explained by the act of secretion it, nevertheless, opens up interesting points for studying the origin of bile.

"We shall hereafter have an opportunity of adducing fresh data for obtaining a deeper insight into the mode of origin of the hepatic secretion, by showing how certain products of metamorphosis disappear within the liver. Bamberger considers that he has found a proof of the preëxistence of bile in the portal vein in the jaundice which is observed to accompany inflammation of that vessel. The blood of the vena porta, which passes by the collateral vessels into the general circulation, it is thought, in such a case produces jaundice by the spontaneous elimination of the elements of bile. During the past year I have met with three cases of complete obstruction of the portal vein, and have availed myself of the opportunity to examine carefully the clot taken from the vessel. Leucine could be detected in it, but not a trace of bile pigment."

Dr. Horace Dobell, of London, in a recent essay, says, "We come next to the special secretion of the liver, that rich, complex, abundant, and important fluid (the bile) poured out at the rate of about two pints every twenty-four hours, not less than thirty-nine fortieths of which is returned to the blood during its passage through the biliary and intestinal tract—in the course of that great and important osmotic circulation constantly going on between the fluid contents of the bowel and the blood."

The amount of fluid poured into the intestines and reabsorbed in twenty-four hours, says Professor Parkes, "is almost incredible, and constitutes of itself a secondary or intermediate circulation never dreamed of by Harvey. The amount of gastric juice alone passing into the stomach and there reabsorbed amounted, in a case lately examined, to nearly twenty-three imperial pints (if we put it at twelve we shall certainly be within the mark). The pancreas furnishes twelve pints and a half in twenty-four hours, while the salivary glands pour out at least three pints; the amount of the bile is probably over two pints. The amount given out by the intestinal mucous membrane can not be guessed at, but must be enormous. Altogether the quantity of fluid effused into the alimentary canal in twenty-four hours amounts to much more than the whole amount of blood in the body. The effect of this continual outflowing is supposed to be to aid metamorphosis; the same substance, more or less changed, seems to be thrown out and reabsorbed until it is either adapted for the repair of tissue or has become effete."

As regards that small portion of the bile (not more than one fortieth of the whole) which is excreted through the intestines, it acts as a potent antiseptic upon the contents of the bowel, stimulates peristaltic action, and in this way, as well as by giving a proper consistence to the feces, assists in their regular discharge, and it rids the system of those waste products of blood and tissue which can no longer yield up nutriment or force to the organism. It is a striking fact that all the digestive fluids, from the saliva downward, promote decomposition until we come to the bile, which, for the first time, reverses the action and interposes an antiseptic, thus maintaining the *status quo* of the proceeds of digestion until they are absorbed or cast off as waste, and staying the evolution of gases in the passage of this waste out of the body."

The above detached extracts of two distinguished modern authors are given to show the views taught to-day by eminent European writers on the liver and its functions. Dr. Frerichs's work is an exhaustive one, and contains a great deal of valuable

information. These writers do not differ materially from the distinguished American physiologist to whose writings I have so often referred.

How comes it that physiologists have endeavored to discover the origin of the bile from so many different sources? How comes it that some claim that it is secreted from the blood brought to the liver by the portal vein; that some claim that it is formed out of the blood brought to the liver by the hepatic artery; that some claim—Dr. Flint, for instance—that it is made out of both arterial and portal blood; and that some maintain that the bile preëxists in the blood, and is separated from it by the liver, as the kidneys are supposed to separate the urea which is constantly floating in the blood?

Where there is an occlusion of the portal vein, or where that large blood-vessel empties into the vena cava, every one who allows himself to think must know that the food and blood which would have gone to the liver, had there been no occlusion, and when the anatomy of the portal vein is natural, must finally reach the liver through the hepatic artery. In the case reported by Mr. Abernethy the hepatic artery was found to be very much enlarged. Certain it is that that blood-vessel can not be said to carry pure arterial blood where the occlusion and malformations exist. The liver acts upon the same materials, though brought to it by a roundabout and indirect channel.

Aristotle, one of the greatest thinkers and most careful observers that ever lived, whose doctrines are still potent throughout Christendom, adopted some absurd notions. Many of his disciples were unwilling to reject these absurdities, though contradicted by their senses and daily observations. And so it is to-day. Anatomists and physiologists have examined, with the scalpel and microscope in their hands, individual organs. Bichat was the only anatomical and physiological examiner and writer who permitted himself to generalize.

Modern scientists have come to agree with Galen, who had no scientific advantages, so-called, that one of the functions of the liver is that of sanguification. As to how any part of sanguifi-

cation is effected in the liver, they have started several fanciful theories. They all agree that the liver does exert some influence upon the food which is continually passing through it, but they do not pretend to know what that something is. Now, I maintain that the something done by the liver is the separation from the food of certain parts of it, which it converts into bile, and that that is the larger part of the bile poured out into the duodenum. I maintain, also, that the liver separates from the blood brought to it from the portal vein the effete matter coming from all of the abdominal organs (the kidneys excepted), and that it converts this effete matter into bile, and that both forms of bile are excrementitious. It is admitted by every writer to whom I have referred that the constituents of the bile can not be found in the blood of the portal vein, and yet, singular to say, they all maintain that thirty-nine fortieths of the bile is continually reabsorbed. I think I have shown clearly that the bile is not formed from pure arterial blood, nor from any one of its constituents—like the saliva, gastric, pancreatic, and intestinal juices.

Prof. Parkes says, "Altogether the quantity of fluid effused into the alimentary canal in twenty-four hours amounts to much more than the whole amount of blood in the body." I think this statement is rather wild. I have shown clearly, in a former chapter, how these several fluids or juices are disposed of by the lungs, the skin, and the kidneys. The bile ought not to be considered in the same connection with them. If physiologists could bring themselves to agree with me, that the bile is formed from materials which ought to be separated from the food and from effete matter, then they would be bound to admit that it is an excrementitious substance. But they have adopted two theories which stand in the way of this agreement; the first is, that all of the food, as soon as it is absorbed, goes at once, and always, to every part of the body to form new tissues; and the second is, that effete matter is continually, and always, separated from the living tissues in the form of urea and other substances. In other words, they all teach that constructive assimilation and destructive metamorphosis, or disassimilation, always go on in an animal

body at one and the same time. They have to dispose of this effete or separated matter, and they have selected the kidneys, particularly, to remove it from the body. They attach no importance to the teachings of comparative anatomy that the liver and the lungs are compensatory organs. They all admit, for they can not avoid it, that the effete matter coming from the abdominal viscera, whether those viscera are in a healthful or diseased condition, must pass through the liver before it can enter the vena cava. They will not permit the liver to separate it from the blood, but allow it to pass on to the left side of the heart, to be thence distributed to every part of the body, when, after many rounds of the circulation, the effete matter finally reaches the kidneys; it is then separated from the blood and passes out of the body. Finding themselves in such a fog, physiologists are compelled to maintain that thirty-nine fortieths of the bile is reabsorbed. The denial that the bile is formed from the food, and from effete matter, involves the doctrine that it is a recrementitious and not an excrementitious substance; that conclusion necessitates the absorption doctrine; that doctrine gave rise to the theory of Claude Bernard that the special function of the liver is to create sugar and fats. These conclusions, taken altogether, led many physiologists to believe that the bile preëxists in the blood, and that it is only separated from the blood by the liver, as they all teach in regard to urea and the kidneys.

Dr. Dobell attaches much more importance to the functional uses of a fractional part (one fortieth) of the bile than does Dr. Flint and many other physiologists. They find cholesterin changed in stercorin in the feces, and therefore they admit that it is not absorbed. My doctrine is that it is all excrementitious, and that it all passes off from the body with the feces. It gives to them color, odor, and consistency. In fevers and other ailments it is the duty of the physician to examine the alvine evacuations, though that is nowadays seldom attended to. The bile is poured into the duodenum and mixed with the food before it is absorbed. That it exerts some influence upon the food preparatory to absorption, I do not doubt. We all know

that a large proportion of what we eat, even in perfect health, is not absorbed, but that it passes off from the bowels as fecal matter. This is a quantity which physiologists have not taken into their calculation, when they balance the *injesta* with *ejecta* in the body of a man which is entirely changed within twenty days. The bile unites with that part of the food which is not to be absorbed, and passes with it from the body. The Gulf Stream passes through the Atlantic Ocean without mixing with the waters on either side of it, and in the same manner the River Rhone passes through the Lake of Geneva. In the same manner the current of bile may pass through the food which it finds in the duodenum. As physiologists can present no proofs that the bile is reabsorbed, they have to resort to an argument to maintain their doctrine that it is not excrementitious. They say that a man can only live a few days if the kidneys fail to perform their functions, but that a man may live for a week or more without any alvine evacuation. Now, I maintain that the bile passes off with the gases. It has been a wonder to me that all physiologists, especially the French, who hardly ever omit to notice any thing, have all omitted to notice one of the excretions from the living body. I mean the gases which escape from the bowels. Were a balloon ever so large not more tightly tied than the sphincter ani, which prevents the escape of gases from the rectum, it would soon collapse. For the most part this exit of gas from the human body is without noise; but from the loudness of the report which is sometimes heard one might suppose the quantity of gas discharged was sufficient to fill a gas-bag holding several gallons. Were a bag of sufficient size attached to the anus of many individuals, and they were thrown into the sea, it would be found that they could not sink. The gases thus escaping from the human body are often villainously offensive. When they have been analyzed physiologists will no longer be at a loss to know what has become of the constituents of the bile. The absorption theory of bile will then be exploded.

It has been estimated that a man weighing one hundred and forty pounds throws off from the lungs by expiration eleven

ounces of solid carbon daily. I do not pretend to be accurate about this. It is certainly a very large amount. Considering the many wonderful experiments which are being made on living animals, it would not be difficult to measure accurately both the quantity of gas which escapes from the bowels and also the solid constituents. These gases are continually escaping and carrying off with them a quantity of solid matter sufficient to account for a large amount of the materials of the bile which physiologists have not been able to discover in the feces. The offensiveness of bed-chambers not well ventilated, and of crowded school-rooms, and of public halls is caused, I think, not so much by the air being breathed over and over again as by the gases to which I have referred.

Has any man ever yet made an investigation to find out the quantity of gas which escapes from the bowels of a man who has no other alvine discharge? It has been the disagreeable task of many physicians to remove scybullæ from the rectum of these costive persons. These hard balls prove that some other discharge has been continually going on. We also find that in some forms of bowel affection a loud discharge of gas from the rectum is the first sign of improvement in the condition of the patient. It is a healthy function as well as an important one. There are some expressions concerning it among the common people which are as forcible and true as are the anecdotes of President Lincoln.

Dr. Dobell says, "The amount of fluid poured into the intestines and reabsorbed in twenty-four hours, says Professor Parkes, is almost incredible, and constitutes of itself a secondary or intermediate circulation never dreamed of by Harvey."

This osmosis is referred to by Dr. Flint. According to my limited knowledge of anatomy, and of physiological laws in healthy bodies, I did not know that any fluids ever escaped from arteries, veins, and lymphatics, except so far as to moisten and lubricate the peritoneal coat investing all the viscera within the cavities of the chest and abdomen. The cuttle-fish when attacked is said to color the waters surrounding him. Physiologists have resorted to this osmosis doctrine for the same reason.

CHAPTER VIII.

THE BLOOD.

If a Lilliputian physiologist had told Gulliver that the blood was formed in the marrow-bones, Swift would have eclipsed Lesage and Molière in sarcasm on the medical profession; yet, wonderful to tell, a distinguished physiologist in the latter part of the nineteenth century has announced such to be the probable fact.

Dr. Flint, in his very learned work, has given some space to the consideration of this new doctrine. Shades of Bacon! What sort of philosophy do you call this? Do you call it the inductive or deductive? In comparison with such authorities, Samuel Thompson, the author of the steam-doctor system, was a Sydenham in medical science.

The same reasoning that led this distinguished savan to suppose that the red blood corpuscles are generated in the marrow-bones, caused some of the most distinguished physiologists of to-day to imagine that the bile was generated in the blood, as it circulates through the arteries, capillaries, and veins. When one false quantity occurs in a calculation, it is impossible to arrive at a correct result; but when half a dozen occur, the doctrine of probabilities itself could not unravel the mystery. Common sense is excluded in both instances.

The microscope has developed infinitesimal mice into mountains of error. The blood is formed in no particular part of the body and by no special organ. It is formed from, and out of, the food. All the glands which empty their secretions into the mouth,

the glands which secrete the gastric juice, the pancreas, and the glands which secrete the intestinal juices, are engaged in its first preparation. These prepare the food for absorption. Physiologists say that the food is absorbed in the form of glucose, albuminose, and fatty emulsion. They detect these substances in the portal blood, and one of them in the lacteals. Whatever else is absorbed, or may be absorbed, they do not know, because the blood is a mixed fluid, and it is difficult to tell what is blood proper, from what has been recently absorbed from the stomach and intestinal canal. The only pure blood that I can conceive of would be that taken from the artery of a strong, healthy, vigorous infant, shortly after birth and before it has sucked. After the child sucks, and before it begins to eat common food, the blood would be comparatively pure. But in grown-up men and women, the blood is so continually mixed up with the food that it would be the next thing to impossible to find a standard pure blood. I should suppose that twelve or fifteen hours after a perfectly healthy person had eaten would be the best time to find it. The purest blood would then, probably, be found in the renal veins, and in the veins of the spleen, just as they come out of that gland.

The absorption of food is but the beginning of the formation of blood. The food has to be subjected to the transforming action of the liver and mesenteric glands. This is the first step after absorption. It is then mixed with the lymph from all parts of the body, before it enters the right side of the heart. It next passes to the lungs, where it is intermixed with the nitrogen and oxygen inspired. The liver and the lungs not only transform the food, to a certain extent, into blood, but they also separate from the food certain substances which they throw off. In the one case it passes off in the form of bile, in the other in the form of carbonic acid and watery vapor. After the food has passed through the lungs, it enters the left side of the heart intermixed with the arterial blood. But the process of converting food into blood is not yet ended. All that which is sent to the spleen—we mean the food, and it is a large quantity—is sub-

jected to a transformative process, in that gland, without having any thing separated from it. That very great and all-important changes are effected in the spleen, we have no doubt. The fact has been noticed that the quantity of albumen and fibrin is increased as the blood passes through the spleen. It is most reasonable to suppose that the spleen vitalizes, so to express it, in a high degree the food as it passes through it, because the blood which flows from the spleen is immediately mixed, in the one case—that is, during digestion—with the food freshly absorbed; and in the other case—that is, when digestion may be said to be completed—with blood which has been impoverished by passing through the abdominal capillary system, and which is returning slowly to the heart as venous blood. Such blood requires the rejuvenating influence of new blood coming from an organ which is so wonderfully elaborate in its mechanism.

But another large portion of the food is sent to the kidneys. It is the office of these organs to separate from the food certain substances, which neither the liver nor the lungs could separate from it. These substances are what are found in the urine of a perfectly healthy person. I have before alluded to, what may be called, elective affinity. It is only a conjecture which, I think, may be worthy of consideration. The supposition is that certain ingredients of the food may go directly to the kidneys, as well as to the skin, for separation. The skin throws off a large quantity of the fluids with which the food is mixed, before it is absorbed, besides a certain quantity of the watery part of the lymph with which all the food is mixed before it enters the right side of the heart. But the food which has recently entered the circulation, does not all go either to the spleen or kidneys after it leaves the left side of the heart. It is carried along by different channels to every part of the body. Before reaching the capillary system of blood-vessels, it is subjected to the lymphatics, which accompany the arteries from their minutest ramifications. It is the office of these lymphatics to separate certain portions of the food, thus distributed over the whole body from the current of the arterial blood, and to subject it to the trans-

formative action of the lymphatic glands. The older physiologists, I believe, considered the lymphatics as the absorbents proper, and supposed that they removed effete matter from the solid organism. I am not here considering the functions of the lymphatics in pathological conditions. I think that they act especially upon the food floating in the arterial blood. I know that urea is found in the lymph. I do not believe that urea is ever a constituent part of the blood of a perfectly healthy individual. I have before said that I think it sometimes escapes into the venous blood coming from the kidneys, when the ducts coming from the glands of those organs fail to pour it all into the ureters, just as the bile is sometimes poured into the hepatic veins, which ought to have gone into the gall-bladder or the duodenum.

It is so seldom that any human being can be called perfectly healthy that it would be almost impossible to say when there may be, or may not be, effete matter floating in the blood. In the training of race-horses and athletes there is waste or loss of weight in their bodies. This waste passes into the veins as effete matter. When there is effete matter in the blood, it may be a question whether the lymphatics have the power to form urea out of it. In pathological states they may exert that power vicariously, but I do not believe that they ever convert effete matter into urea when the kidneys perform their normal functions healthfully. Physiologists, so far as I have been able to inform myself, have no definite knowledge as to the office or functions of the lymphatics in the animal economy. Nor have they, I think, any better knowledge of the uses of the lymph itself. The food is either converted into solid tissue immediately after its absorption, as taught by physiologists generally, or it is subjected for many hours to the action of all the glands referred to above in its transformation into blood. This is my belief. After its absorption it is subjected to the action of certain glands already referred to. But inasmuch as a large portion of the blood, carrying the food along with it, does not go immediately to these glands, but is distributed to all parts of the body,

ought it be thought strange that a particular system of absorbing vessels—the lymphatics—should accompany the arteries distributed over the whole body, with the duty imposed on them of absorbing the food floating in these arteries, and of subjecting it to the transformative power of their glands? I have said that it was the office of the spleen to pour into the portal vein containing the food recently absorbed, or containing blood somewhat impoverished, what I assume to call rejuvenated blood. The lymph is known to contain in large quantities highly vitalized principles. This lymph is mixed with the oily and fatty portions of the food as soon as it enters the thoracic duct. In both instances during digestion we have in one case fresh blood from the spleen mixed with the food, and in the other a highly elaborated fluid—the lymph—mixed with another portion of the food. These intermixtures constitute one link in the chain for the conversion of the food into blood. I believe the blood is formed *in transitu* by the action of the various glands already referred to. The white blood corpuscles antedate the red disks. Physiologists may be able to discover the glands which perform the most important part in the fetal life, so to express it, of white and red blood corpuscles. They have not yet been able to do it. What is the period of life of these corpuscles, perhaps, can never be determined. No doubt, after a shorter or longer time, they are thrown off from the body. When that time comes they form a part of what I call effete blood.

I have presented in the foregoing pages what I believe to be a correct view of the secretions and excretions. Excepting those concerned in generation and reproduction, the secretions and excretions are wholly and exclusively concerned in the conversion of food into blood, and of this blood, during growth, or when the body is increasing in weight, into solid organized tissues. This is the labor of life, so far as the vegetative and organic processes are concerned. After the food has been converted into blood, each tissue (I again mean the tissues of Bichat—muscle, bone, etc.) selects from the blood the parts of it necessary to perfect its organism and to repair waste, when there is any waste.

This conversion of food into blood causes corresponding motion, which motion evolves animal heat. Animal heat, during good health, is generated in all parts of the body wherein there is motion. This is the explanation why men living in warm climates eat comparatively little food, and only such food as requires little labor or motion in converting it into blood. On the other hand, it explains why men living in cold climates eat very large quantities of food, and such food, raw meats, oils, and fats as requires long-continued labor and motion in the conversion of them into blood.

That the nervous system is largely concerned in all the processes of digestion there can be no doubt. In man, who may be called the most highly developed animal, each system and subsystem is dependent the one on the other. That the first impressions, and the most powerful which can be made upon man, are made through the nervous system, I think there can be no question; but to produce healthy nervous fluid there must be healthy or pure blood. A healthful and vigorous condition of the nervous system can not exist unless the system of vegetative or organic life is also in a healthy state. So, likewise, a disturbed condition of the nervous system may, and often does, interfere with or interrupt the organic and vegetative system.

CHAPTER IX.

HYPEREMIA AND CONGESTION.

These two words, hyperemia and congestion, are used as synonymous terms by both Drs. Dalton and Flint in their learned and elaborate works on Physiology. They are constantly so used by medical writers generally. Venous congestion is always, in my opinion, a pathological state. Arterial congestion, or hyperemia, may exist under both a physiological and a pathological condition. Arterial congestion may exist pathologically in the formative stage of inflammations, and in fevers, but the fullest expression of hyperemia, in the proper sense of that word, is expressed in the blush on a maiden's cheek and in the orgasm of erectile organs, both of which are physiological conditions.

Hyperemia, in my opinion, should never be applied to venous congestion. The doing so causes confusion. The word should be restricted entirely to mean a purely physiological state or condition. When we mean to speak of *arterial congestion* under pathological conditions, we should so express it. We should not in such a case use the word hyperemia. There is, in my opinion, no pathological condition so little understood and about which there are so many confused opinions as *venous congestion*. John Estlin Cook, M. D., the distinguished professor of the theory and practice of medicine in Transylvania University, at Lexington, Ky., was the first man who ever understood fully what venous congestion means. He was certainly the first medical writer who made the thing intelligible. It would be impossible for me to do better than copy exactly and literally what he has written upon

the subject. The extracts will be found in the second volume of Cook's Pathology and Therapeutics, beginning at page 3 and paragraph 1209, and ending at page 13 and paragraph 1250, inclusive:

"1209. In the preceding pages we have seen that weakened action of the heart is one link of the chain of causes and effects extending from the remote causes to the symptoms of fever (18, 691). The next question in the prosecution of this inquiry (19) is, what is the effect of this cause, weakened action of the heart?

"1210. One consequence immediately flowing from weakened action of the heart, is the diminution of the quantity of blood sent into the arteries; whence follows, first, weakness of the pulse, a very obvious consequence of the diminution of the power which distends the arteries; second, paleness and coldness of the surface, consequences as certain of the failure of the usual supply of blood to the surface; third, diminished bulk of the external parts, shrinking of the features, and shriveling of the skin, all necessarily following deficient fullness of the cutaneous and sub-cutaneous vessels. Weakness of the pulse, paleness, and coldness of the surface, and diminished bulk of the external parts, shrinking of the features, and shriveling of the skin, are therefore immediate consequences of diminished action of the heart. There is another consequence of this state of the heart, very extensive in its operation on the human system.

"1211. The heart is an exceedingly strong muscular cavity, which receives the blood from the veins, and impels it with great force and velocity into the arteries.

"1212. The arteries are firm elastic tubes, the greater part of them firm enough to retain the form of a tube when empty. At every contraction of the heart the blood is forced into them, whereby they are distended; but they have the power of contracting so forcibly as immediately to overcome the distension. This distension is sometimes visible. It may be seen in the temporal artery in those thin persons in whom that artery is very prominent, running, as it were, above the level of the neighbor-

ing skin. It may be distinctly ascertained by feeling the radial artery when the pulse is strong. At the moment of distension the artery is felt to swell out to the fingers, and immediately after to retire from them; and this is the case when the artery is so strictly confined between the radius and the fingers that very little more pressure will obstruct the passage of the blood, so that the artery is incapable of any departure from the straight line. The arteries have the power of expelling the blood contained in them, when the heart ceases to act, so that after death they are found empty. I have seen the external iliac so closed as to require the insertion of a probe to show the cavity.

"1213. The veins are flaccid vessels, exerting little force on their contents, and readily yielding to a distending force.

"1214. The divisions and minute subdivisions of the arteries and veins penetrate every part, and constitute a large portion of every structure, and almost the whole of some, as the glands. Through these numberless vessels the blood, with great force and rapidity, is necessarily driven; a mass equal to the whole quantity in the body, passing from the arteries to the veins many times every hour. The heart propels the blood into the arteries, the arteries propel it into the veins, which slowly return it to the heart. Whilst the heart acts vigorously the vital stream is passed on as fast as it presents itself; but as soon as the action of that organ is diminished, less is received, and the great veins are distended. If the diminution of the action of the heart be sudden, in a little time the accumulating blood presses on the heart and stimulates it to increased action, whereby the evil is sometimes remedied. But the continued operation of the causes in force sufficient to produce continued weakness of the action of the heart produces continued fullness of the great veins.

"1215. Early in life the balance of the circulation is maintained by the activity of the heart; but as soon as it begins to flag, the scale is turned against the veins. As age creeps on, venous plethora becomes more and more apparent; and when the action of the heart ceases in death, the veins are found full, the arteries empty.

"1216. This is not the effect of age alone. In every period of our existence, when disease puts an end to the action of the heart, the result is the same. The vena cava is found full.

"1217. Nor is it the effect of disease. In all the vigor of life, if by any operation the action of the heart be stopped, the immediate consequence is the same. 'When the breast of a living dog is opened by taking away the sternum with the cartilaginous appendices of the ribs, the lungs are observed suddenly to sink, and afterward the circulation of the blood and the motion of the heart to cease. In a little time after that the right ventricle of the heart and the vena cava are swelled as if they were ready to burst.'

"1218. The effect of weakened action of the heart, therefore, is accumulation of blood in the great veins entering into it.

"1219. As the latter is the consequence of the former, that which indicates the former indicates also the latter. We have before seen (82, 83) that a pulse smaller and weaker, and a surface and extremities paler and cooler than usual, indicate weakened action of the heart—these symptoms, therefore, also indicate accumulation of blood in the vena cava and its branches.

"1220. As this accumulation necessarily extends to the great branches of the cava, let us inquire how far it extends and what branches are readily filled.

"1221. The veins have but little action; they derive assistance from various sources to enable them to pass on their contents. As in different parts of the system this is more or less effectual, or as they are more or less disposed to yield, accumulation of blood to a greater or less extent must take place.

"1222. The veins of the fleshy part having the benefit of almost perpetual action of the muscles, and of the aid of valves, to prevent regurgitation and to give the more complete effect to the muscular pressure, the blood has but little opportunity to accumulate in them, but is incessantly driven to the interior of the body.

"1223. The vena cava, and its branches between the last valves and the heart, being destitute of those aids which in other

parts help the blood along, must be the chief seat of venous plethora.

"1224. This great cavity consists of the vena cava, the external iliac, and the crural veins, the hypogastric or internal iliac veins and their branches, the hepatic veins and their branches, the vena portæ and its branches, the renal veins, the subclavian veins, and the internal and external jugular veins.

"1225. The veins of the arms escape from among the muscles near the head of the humerus, and joining with others from the neighboring parts, form the subclavian. This point is the limit of the cavity in this direction.

"1226. The veins of the head, internal and external, including the sinuses of the brain, have no valves; and therefore the cavity in this direction extends to the origin of the veins, and includes those passing from the glands about the head. These all have free communication with one another, and with the cava through the subclavian vein.

"1227. The cavity is limited by strong valves at the mouth of the azygos, the next considerable vein entering the cava.

"1228. The coronary veins of the heart are in like manner strongly guarded.

"1229. The hepatic branch of the venous cavity is very extensive. The vena portæ, which, ramifying in the liver by the reunion of its numberless branches, forms the venæ hepaticæ, is composed of three large veins, besides some minor ones from the stomach, the duodenum, and the gall-bladder. These are the greater mesenteric, the splenic, and the internal hemorrhoidal or smaller mesenteric veins.

"1230. The greater mesenteric vein is formed of the numerous vessels which return the blood from the small intestines, the cecum, the right portion of the colon, the pancreas, and the omentum. By the convergence of all these is formed an immense assemblage of veins of very large size in the mesentery, which anastomose in the freest manner. It is difficult to realize their number and their size without examining a good preparation.

"1231. The splenic vein carries into the vena portæ the blood

sent to the spleen, a very considerable quantity. The size of this vein in comparison with the artery is peculiar; its trunk is to that of the splenic artery in the proportion of five to one, and consequently there is a proportionate slow motion of the blood in the veins.

"1232. The internal hemorrhoidal or smaller mesenteric is formed of the veins which return the blood from the upper part of the arch and the left portion of the colon, and from the whole of the rectum. Even these remote branches are a part of the venous cavity. When the smaller mesenteric vein of a subject having piles is dissected the ramifications terminate in these pouches of blood. Hence the mortal hemorrhages from these small tumors which have occasioned so much surprise.

"1233. The structure of this extensive system renders it in a remarkable degree liable to suffer accumulation of blood. The coats of the veins, especially those of the spleen and liver, are weak, and exert little force on the fluid contained in them; they are little assisted by muscular action; they have no valves; their great peculiarity, the subdivision of the vena portæ in the substance of the liver and the reunion of its small branches into large veins, called hepatic, promotes accumulation of blood in them. In every other part of the system this fluid, after having passed through one set of veins, receives anew the impulse of the heart. Here it passes through two additional sets before it has that advantage. Hence the blood is, in these vessels, extremely sluggish in its motion.

"1234. The large but short renal veins also form a part of the venous cavity.

"1235. The hypogastric or internal iliac veins are very large branches of the cava; the internal iliac artery carrying into the pelvis nearly as much blood as the external iliac does to the lower extremities.

"1236. The uterine branches of these veins are large and capable of extraordinary dilatation. Wistar says the veins of the uterus, during gestation, are in some places more than half an inch in diameter.

"1237. Other branches of these veins return the blood from the whole substance of the back part of the pelvis, viz. the parts within and without, and from the cavity of the bone itself.

"1238. The external hemorrhoidal veins are branches of these, and when distended constitute the external piles. Thus the piles, whether internal (1232) or external, are portions of the venous cavity.

"1239. This branch of the great venous cavity lies lower than any other part of it, except the lower extremity of the external iliac vein; and is therefore pressed by a great weight, the pressure of a column of fluid being in proportion to its height. Freely communicating with the cava, it partakes of its fullness whenever the blood accumulates in that great vein.

"1240. A short distance without the abdomen, a considerable number of veins from the large muscles of the leg and thigh uniting, form the crural vein, which, in proportion to any of the rest, is quite large; and after entering the abdomen takes the name of external iliac vein. The smaller branches which form the crural vein, being exposed to the constant action of the strong muscles of the thigh, and having the aid of valves, are not liable to partake of any ordinary degree of fullness of the vena cava. Here, then, is the limit of the venous cavity in this direction.

"1241. These branches of the cavity vary much in their disposition to yield, and in their sensibility under venous plethora. The hepatic is the weakest in its structure, and most apt to yield. The veins constituting the bulk of the liver and spleen have been so excessively distended, and thereby weakened, that these viscera, on being taken out of the body immediately after death, have been found unable to bear the weight of their contents and have fallen in pieces from the hands of the operator. The uterine branch of the hypogastric vein is capable of great dilatation; but the head is most sensible to fullness of its veins.

"1242. This is not always found to be the relative disposition in the different branches of the venous cavity, to yield to the

accumulation of blood. In some instances the veins of the head give way more readily than usual. In others, the uterine veins, though ordinarily much disposed to yield, are extremely rigid. In a third set, you may find the liver or spleen habitually tumid, and, when reduced to the ordinary size, prone to become large again.

“1243. In order to have a distinct idea of the cavity, let us suppose the action of the heart suddenly to cease. The check given to the blood would instantly produce distension of the cava (1218), which would, in every successive moment, dilate successive portions of that vessel and its branches till the valves were raised. It is evident that at this moment the regurgitated blood would distend the cava, the internal and external jugular veins, the subclavian, the hepatic, the renal, and the hypogastric veins, and the external iliac and crural veins; and that access would be denied to it, into the veins of the limbs, and into the azygos, and the coronary veins to the heart.

“1244. The following dissection of a child five or six months old was made with a view to this subject. The ribs, sternum, and clavicles, the integuments and muscles of the arms and legs, the diaphragm, the mediastinum, the stomach, were all cut away, so as to show the whole of the veins from the calf of the leg and the upper part of the arm into the cava and the whole course of the cava ascendens and descendens with the subclavian veins. It was, when thus laid open to view, at a single glance apparent that the right side of the heart, the cava, the subclavian veins, the iliac, the crural, and the deep veins running along with the femoral artery down as far as the calf, were full of black blood. The veins were so full as to retain the round shape, and were stuffed full to the valves near the head of the humerus, at which point the stuffed appearance ceased suddenly; and immediately at the termination the vein returning from the arm was empty, flaccid, and transparent, so as scarce to be perceived by candle-light. On pressing the blood of the full venous cavity toward the stuffed end at the head of the humerus, it would not pass into the vein beyond the valves; but on increas-

ing the pressure, it was forced out of the minute veins, entering into the subclavian from the skin and other neighboring parts, which had been cut across in laying bare the subclavian vein, and which were so minute as not to let the blood pass till pressure was made. The venous cavity was here shown in its exact shape, etc. The azygos had blood in it, but was quite small.

"1245. Though this cavity be the special seat of venous plethora, there are cases in which the gradual accumulation of blood is such that all the veins of the body are distended. In some of these cases the veins even on the back of the hand are singularly prominent, and so distended as to be quite round.

"1246. In some cases, too, when in consequence of general plethora and great inaction, pregnancy, or other obstruction, the column of blood in the saphena so distends that vessel that its valves are no longer capable of meeting and closing the passage. This venous trunk has no barrier between it and the venous cavity, and becomes a part of it.

"1247. It is evident that repeated distensions of the venous cavity must lessen its tone and render it more liable to yield in future and even to remain permanently enlarged; and that this enlargement must especially affect those parts that are particularly lax in their structure; as the vessels of the liver, the spleen, and the uterus.

"1248. Accumulation of blood in the venous cavity being the effect of weakened action of the heart, is another link of the chain of causes extending from the remote causes to the symptoms (18). The question is (19), what are the effects of this cause, accumulation of blood in the venous cavity?

"1249. Before we proceed, however, some things must be noticed which produce this link of the chain of causes, and are therefore remote cause of fever (1204).

"1250. The suppression of customary evacuations is an obvious cause of fullness of the vessels, which, if the action of the heart continue the same, must center in the venous cavity."

Dr. Cook's book is out of print. The first volume is devoted to the explanation of his views as to the miasmatic or malarial

origin of fevers and other maladies. Of this part of his work Dr. James Conquest Cross, a distinguished teacher of medicine in the Medical College of Ohio at Cincinnati, and afterward in Transylvania University, said that it was useless for any man ever to attempt to write any thing further upon the malarial origin of diseases, because Dr. Cook had exhausted the subject. Not one in ten thousand of the medical men of to-day ever saw a copy of Dr. Cook's work, and yet thousands of medical writers, considering the question from various standpoints, declare malaria to be the remote cause of many maladies. Their views and opinions were anticipated by Dr. Cook over sixty years ago. He has clearly explained what they so vaguely understand. Since his day Daniel Vaughan, of Cincinnati, has written the only essay on this subject which is worth any thing. His essay was published some years since in the Dublin, Edinburgh, and London Philosophical Magazine, and afterward in the Cincinnati periodicals.

If any one will read Dr. Murchison's valuable little work on Functional Derangements of the Liver, he will find that Dr. Cook has, in his second volume, anticipated him almost in every particular, and explained the doctrines taught by Dr. Murchison much more clearly and distinctly than Dr. Murchison was able to do in his own book.

The above extracts from the works of Dr. Cook have been submitted to one of the ablest teachers of anatomy in the United States, who is also one of the most distinguished and successful physicians and surgeons in this or any other country. He says that the anatomical and post-mortem facts observed and noted by Dr. Cook are exactly and positively true.

I hope the reader will give the closest attention to them, and study them well before he proceeds further. There is no understanding of disease, and particularly of the uses of emetics and cathartics, without a knowledge of congestion as explained by Dr. Cook. This applies more particularly to the action of the liver, the removal of effete matter, and to the therapeutical effects of calomel. Without this knowledge of congestion

neither the action of the liver in certain forms of disease, the removal of effete matter, nor the curative uses of calomel can be understood.

As remarked by Dr. Cook, and as known by every body acquainted with the subject, the arteries are found empty after death, and the veins filled with blood. This shows how distensible the veins are, and that they are capable of holding all the blood in the body. It is because of the fact that the arteries are found empty after death that they were supposed to contain air during life, which was the popular belief until Harvey discovered the circulation of the blood.

This excessive fullness of the veins with a lesser quantity of blood in the arteries can only be approximative during life. The more prolonged and complete the weakened action of the heart is, the more excessive will be the congestion of the vena cava and its branches. As long as the action of the heart is continued, some blood is distributed through the arteries to all parts of the body. Of course there is a corresponding return of blood through the veins to the heart. But so long as there is vitality enough left for the valves within the veins to act, all veins having valves empty themselves, and are not, and can not be, the subject of venous congestion.

Hence it is that while the veins within the cavities are congested, as explained by Dr. Cook, those upon the surface are comparatively empty, carrying off only the small quantity of blood brought to them by the arteries. The fullest expression of this internal congestion, particularly of the abdominal viscera, with the comparatively empty condition of the superficial veins, is found in the collapse of cholera.

If a dam is built across a creek or a little river, the bed of the creek or river first fills up with water, after that all the little rivulets which empty into this pond or lake become filled to some extent with the back water which ascends their descending currents; so it is also, as explained by Dr. Cook, with the veins having no valves, which empty into the vena cava. The seat of this congestion, as also explained by him, is particularly in

the abdominal and pelvic viscera and within the cranium. Excessive congestion of the internal and external jugulars, and of the sinuses of the brain, is prevented to some extent by the respiratory movements. So long as the respiration is natural and quiet, the movements of the chest hasten and assist the venous circulation; but in forced or laborious respiration they do not assist, and may even retard its flow. This explains why, up to a certain stage of cholera, there is not much venous congestion of the brain, and the intellect remains clear; while afterward when the respiratory efforts are much interrupted, the congestion of the veins of the brain is excessive, and the countenance becomes livid. So long as the respiratory movements are uninterrupted, venous congestion of the brain is, to some extent, prevented.

In all such cases the most excessive distension or congestion of the veins will be within the abdominal cavity, particularly in the veins which make up the portal vein and in the veins distributed through the liver. This is the condition of things in cholera, and the congestion of the cava, and of the veins having no valves emptying into it, is more excessive as collapse approaches. The veins of the heart escape this excessive engorgement of the great venous trunks, because they have valves. The heart, under the condition of things supposed, could not work at all if its own internal circulation was interfered with; that is, the circulation of the coronary arteries and coronary veins. The lungs also are beyond the range of congestion or engorgement of the great venous trunks, because of a valve at the mouth of the azygos veins. While the blood continues to flow, the heart beats, though ever so feebly; and although but a small quantity of blood passes through it, this small quantity even is assisted on its way to the lungs by passing through an artery, the pulmonary, having within itself contractile power. The mouth of the pulmonary artery is guarded by valves, which prevent the regurgitation of blood from the right side of the heart, though filled to excess. Wonderfully wise provisions in the economy of Nature and greatly to be

admired! But the venous azygos also has strong valves at its mouth. The superior vena cava receives the vena azygos major just before it enters the pericardium.

“The azygos veins connect together the superior and inferior venæ cavæ, supplying the place of these vessels in that part of the trunk in which they are deficient on account of their connection with the heart. The larger—the *right azygos vein*—commences opposite the first or second lumbar vertebra by receiving a branch from the right lumbar veins, sometimes by a branch from the renal vein or from the inferior vena cava. It enters the thorax through the aortic opening in the diaphragm, and passes along the right of the vertebral column to the third dorsal vertebra, where it arches forward over the root of the right lung, and terminates in the superior vena cava just before that vessel enters the pericardium. Whilst passing through the aortic opening of the diaphragm it lies with the thoracic duct on the right side of the aorta, and in the thorax it lies upon the intercostal arteries, on the right side of the aorta and thoracic duct, covered by the pleura.

“*Branches.*—It receives nine or ten lower intercostal veins on the right side, the vena azygos minor, several esophageal, mediastinal, and vertebral veins; near its termination, the right bronchial vein, and it is occasionally connected with the right superior intercostal vein. A few imperfect valves are found in this vein, but its branches are provided with valves. The intercostal veins on the left side, below the two or three upper intercostal spaces, usually, from the trunks, named the left lower, and the left upper azygos veins.

“The *left lower or small azygos vein* commences in the lumbar region by a branch from one of the lumbar veins or from the left renal. It passes into the thorax through the left crus of the diaphragm, and, ascending on the left side of the spine as high as the sixth or seventh vertebra, passes across the column behind the aorta and thoracic duct to terminate in the right azygos vein. It receives the four or five lower intercostal veins of the left side and some esophageal and mediastinal veins.

"*The left upper azygos vein* varies according to the size of the left superior intercostal. It receives veins from the intercostal spaces between the left superior intercostal vein and highest branch of the left lower azygos. They are usually two or three in number, and join to form a trunk which ends in the right azygos vein or in the left lower azygos. When this vein is small, or altogether wanting, the left superior intercostal vein will extend as low as the fifth or sixth intercostal space.

"*The bronchial veins* return the blood from the substance of the lungs; that of the right side opens into the vena azygos major, near its termination; that of the left side in the left superior intercostal vein." (Anatomy, Descriptive and Surgical, by Henry Gray, F. R. S.)

I have said, in a former chapter, that although the finding of valves in the veins by Fabricius was considered to be a very important discovery, no importance had been attached to the uses of those valves by writers on practical medicine. Their uses seem to have been ignored from the time of Fabricius to that of John Estlin Cook. Since his day they have been barely alluded to by writers on the practice of medicine. A knowledge of the anatomy of the azygos vein is necessary to understand the uses of valves in the veins and Dr. Cook's theory of congestion. Hence the extract above from Gray's Anatomy.

Teachers and writers on the practice of medicine are too much engaged in the study of microscopic appearances (histology) to attach any importance to such a quantity as valves in the veins, and valves at the mouth of the azygos veins in particular.

It will be seen from the above description of the azygos vein and its branches, that the veins coming from the spinal column, and the parts embraced under that general term, as well as the veins coming from the muscles engaged in the respiratory movement, and from the lungs themselves, having valves in them, these parts can not be directly affected by excessive congestion of the vena cava and its branches, because the blood can not regurgitate back into the vena azygos. The respiratory movements on which life depends as much as upon the action of the heart itself,

are thus protected from a disturbing cause in the animal economy, which itself threatens death.

Venous congestion of the spinal column is also thus prevented to a great extent.

Thus we see clearly and distinctly that the part of the venous system which is particularly the seat of excessive congestion, is the vena cava and its branches, which have no valves in them. We have also noticed that venous congestion of the brain is, to some extent, limited by the respiratory movements; so that, except the main trunks of the venæ cavæ, it is the numerous branches found within the abdominal and pelvic cavities which are the receptacles of this excessive amount of blood.

I have taken cholera as the type to express a form of disease, one of the most remarkable features of which is excessive abdominal venous congestion. The body is wasting rapidly. The aqueous portion of the blood is being poured out from the arteries distributed to all of the abdominal organs in the form of copious rice-water discharges. (I have yet to speak of the capillary system in this connection, but must now pass by it.)

By this loss of its watery parts the blood becomes thickened, and its motion through the abdominal veins is more sluggish and retarded. The blood may be said to have become, in part, effete. Destructive metamorphosis is rapidly going on, and effete matter proper is continually passing from the capillary system into the venous trunks. But a limited quantity passes through the lungs in the stage of cholera now referred to; so that the blood is not revived as usual in its passage through those organs. Animal heat is growing rapidly less, the breath being colder than the external air. The spleen ceases to rejuvenate the blood passing through it, being itself the seat of excessive congestion.

(I have yet to notice the condition of the nervous system.)

As has been so repeatedly stated, all the blood in the abdominal organs is carried by the veins to the liver. It has no other way to get back to the heart. The reader must see at a glance what kind of blood the liver has to act upon in a cholera patient. The quantity is excessive, the quality is vitiated in every manner

in which it can be, both by subtraction and addition; and the motion of this blood through the liver is unusually slow and sluggish. All of this is coincident with and depending upon weakened action of the heart. The work thrown upon the liver is so overwhelming, and the blood upon which it has to work is so deteriorated that it stops action altogether, and no longer works at all.

From the first appearance of rice-water discharges the liver ceases to separate bile from the blood. Unless this function of the liver is restored by some means death by cholera is inevitable. No man ever yet saw a case of cholera begin to improve or to recover until the liver had recommenced the separation of bile from the blood and poured it into the intestines, as evidenced by the stools. To relieve this state of things, calomel is the all-efficient remedy. It will, given in sufficient doses, just as certainly make the liver separate bile from the blood in all curable cases as two and two make four. But I have not yet come to the treatment of cholera. Dr. Cook explains in detail how the venous congestion is observed in various forms of disease. I am not writing a treatise on the practice of medicine, but am only endeavoring to explain certain physiological and pathological facts, and the uses of particular medicines which grow out of a correct understanding of those facts.

I have said that in the collapse of cholera we have the fullest expression of venous congestion. In all other forms of disease it is approximative in degree. In all forms of malarious, miasmatic, or autumnal fevers it exists in certain stages. In the cold stage of intermittent or ague and fever, and particularly in what is known as congestive fever in the South, the internal venous congestion is almost as complete as it is in cholera. In this stage of these fevers the action of the heart is excessively weakened, and the congestion of the vena cava and its branches is great; but fortunately in intermittent fevers, and also in congestive fevers, which do not terminate fatally, in one of its cold stages, after a shorter or longer interval, the heart recovers its lost power, and the circulation is again fully established, the blood

being freely distributed through both arteries and veins. We have, indeed, increased motion in the current of the circulation, which we call fever, attended with an excess of animal heat. This fever is followed by perspiration or sweating, which is sometimes excessive. During this sweating stage the action of the heart again becomes weak, causing a corresponding congestion of the venous cavity, but to a less extent than in the cold stage.

Miasmatic fevers of an intermittent type are frequently of long-continued duration, and the constant recurrence of congestion in the abdominal organs causes it sometimes to become permanent, and we have enlarged spleen, with a fullness of the portal veins generally, and of those of the liver in particular. Here, again, we have all-important work for the liver to perform. Miasmatic fevers go under the general head of bilious fevers. I know that there is no class of diseases in which quinine is so efficacious in its curative effects as in fevers produced by miasm or malaria; but I also know that there is no class of fevers in which the judicious use of emetics and cathartics is more serviceable. The liver must be continually aroused to action to remove this constantly returning congestion. This it does by separating effete matter from the blood and converting it into bile. Fevers of this class have existed from time immemorial, and physicians have always and every where observed that the secretion or excretion of bile is always suspended during their continuance unless some means are employed to remove the difficulty. Where quinine or other anti-periodics alone are relied upon, the cure is incomplete, and the individual is generally left in a condition of chronic ill health.

Neuralgia and other forms of nervous complaint, with indigestion or some form of dyspepsia, are far more frequent now than they were formerly, when more attention was paid to the secretions of the liver in the treatment of these forms of fever. Congestion of the venous system within the cavities, as explained by Dr. Cook, is also of frequent occurrence in many forms of chronic disease.

The judicious use of medicines which cause the liver to separate bile from the blood is more certainly curative than any other method of cure in the treatment of this class of affections. Two things are accomplished by this method of cure: first, effete matter, which is constantly increasing under this state of things, is removed from the blood; second, the heart's action and the circulation of the blood are made to become regular and healthful. It will be noticed, as stated by Dr. Cook, that the renal veins empty into the vena cava, and therefore become congested when that great vein and its branches contain an unusual amount of blood. The renal veins have no valves. This congestion of the renal veins incapacitates the kidneys for the healthful performance of their functions. When an individual is suffering from any form of miasmatic fever the urinary secretions are always irregular in quantity. The kidneys, acting alone upon arterial blood, exert no influence whatever upon the large quantity of blood congested in the vena cava and its branches. To talk about their removing effete matter from the blood, which exists principally in this accumulated congested blood within the abdominal veins, is absurd. Healthy urine will be secreted or excreted in these cases when the liver secretes bile freely, and not until then. A proper cathartic is the best diuretic in all such cases.

I could instance many other forms of disease, where these congestions depending upon weakened action of the heart are of constant occurrence, but I must pass on. There are other disturbances growing out of these congestions which I might also notice, but I must defer their consideration. The congestion of the venous cavity, as explained by Dr. Cook, is not a chimera, it is not a supposition, it is not a theory as that word is commonly understood; but it is what *must* take place when the action of the heart becomes weakened, and when a full, healthy, and regular current of blood does not pass through that organ. In the maladies immediately referred to above there is either an entire loss of appetite, or it is depraved, and digestion, in the full meaning of that word, is irregular and imperfect. Destructive

metamorphosis, or waste, is the consequence of this imperfect digestion and of the morbid influence constituting the disease. Effete matter is separated from the solid organism in large quantities, and entering first the venous system it is contained chiefly in the vessels of the congestive venous cavity. If the liver is made to act, this effete matter is separated from the blood by the liver and is poured out in the form of excrementitious matter, called bile. But if the liver does not act, as is too often the case in the maladies already referred to, this congested blood, containing effete matter, passes slowly through the liver and is carried on through the vena cava, ascending to the heart. It is then mixed up with the arterial blood, and the whole animal economy, particularly the venous system, suffers in consequence. It does not require a microscope to see these things. They are facts which can not be explained away. The difficulty can not be removed except by secretion, call it excretion if you please, and the liver is the secernent organ by which this morbid effete matter is secreted from the blood and thrown off from the body. The kidneys, as above remarked, not acting upon the venous blood, have little or nothing to do with the removal of effete matter from it. If the liver is unable to act, and effete matter passes into general circulation, then, possibly, the kidneys may remove some of it from the blood, as they remove foreign or medicinal substances which are sometimes swallowed; but they accomplish almost nothing in the removal of venous congestion.

I have once said that the older medical writers supposed that the lymphatics were the absorbents proper, and that they removed effete matter from the solid organism. Besides what I have already said in regard to the lymphatic system, I would call the reader's attention to the fact that the lymph is not carried directly to the secernent organs, as the liver, kidneys, or skin; but, in health, is first mixed with the food coming from all sources before it reaches the right side of the heart, and in all cases is poured into the cava near the right auricle. In my opinion, the capillary system presides over, and controls, absorption. It is the system through which both destructive and

constructive metamorphosis is effected. I will consider this system briefly, as it is connected with hyperemia and congestion.

I copy from Dr. Dalton's Physiology sufficient to make intelligible the few remarks I have to make on the capillary system:

"The capillary blood-vessels are minute inosculating tubes which permeate the vascular organs in every direction and bring the blood into intimate contact with the substance of the tissues. They are continuous with the terminal ramifications of the arteries on the one hand, and with the commencing rootlets of the veins on the other. They vary somewhat in size in different organs and in different species of animals; their average diameter in the human subject being a little over $\frac{1}{3000}$ of an inch. They are composed of single, transparent, homogeneous, somewhat elastic, tubular membrane, which is provided at various intervals with flattened, oval nuclei. As the smaller arteries approach the capillaries, they diminish constantly in size by successive subdivision, and lose first their external or fibrous tunic. They are then composed only of the internal or homogeneous coat, and the middle or muscular. The middle coat then diminishes in thickness, until it is reduced to a single layer of circular, fusiform, unstriped, muscular fibers, which in their turn disappear altogether as the artery merges at last in the capillaries, leaving only, as we have mentioned, a simple homogeneous, nucleated, tubular membrane, which is continuous with the internal arterial tunic.

"The capillaries are further distinguished from both arteries and veins by their frequent inosculation. The arteries constantly divide and subdivide as they pass from within outward, whilst the veins as constantly unite with each other to form larger and less numerous branches and trunks as they pass from the circumference toward the center. But the capillaries simply inosculate with each other in every direction in such a manner as to form an interlacing network or plexus, the *capillary plexus*, which is exceedingly rich and abundant in some organs, less so in others. The spaces included between the meshes of the capillary network vary also in shape as well as in size in different

parts of the body. In the muscular tissue they form long parallelograms; in the areolar tissue, irregular, shapeless figures, corresponding with the direction of the fibrous bundles of which the tissue is composed. In the mucous membrane of the large intestine the capillaries include hexagonal or nearly circular spaces, inclosing the orifices of the follicles. In the papillæ placenta they are arranged in long spiral loops, and in the adipose tissue in wide meshes, among which the fat vesicles are entangled.

“The *motion of the blood in the capillaries* may be studied by examining under the microscope any transparent tissue of a sufficient degree of vascularity. One of the most convenient parts for this purpose is the web of the frog’s foot. When properly prepared and kept moistened by the occasional addition of water to the integument, the circulation will go on in its vessels for an indefinite length of time. The blood can be seen entering the field by the smaller arteries, shooting through them with great rapidity and in successive impulses, and flowing off again by the veins at a somewhat slow rate. In the capillaries themselves the circulation is considerably less rapid than in either the arteries or the veins. It is also perfectly steady and uninterrupted in its flow. The blood passes along in a uniform and continuous current, without any apparent contraction or dilatation of the vessels, very much as if it were flowing through glass tubes. Another very remarkable peculiarity of the capillary circulation is, that it has no definite direction. The numerous streams of which it is composed do not tend to the right or to the left, nor toward any one particular point. On the contrary, they pass above and below each other, at right angles to each other’s course, or even in opposite directions, so that the blood, while in the capillaries, merely circulates promiscuously among the tissues in such a manner as to come immediately in contact with every part of their substance.

“The motion of the white and red globules in the circulating blood is also peculiar, and shows very distinctly the difference in their consistency and other physical properties. In the larger

vessels the red globules are carried along in a dense column in the central part of the stream; while near the edges of the vessel there is a transparent space occupied only by the clear plasma of the blood, in which no red globules are to be seen. In the smaller vessels the globules pass along in a narrow column, two by two, or following each other in single file. The flexibility and semi-fluid consistency of these globules are here very apparent, from the readiness with which they become folded up, bent, or twisted in turning corners, and the ease with which they glide through minute branches of communication, smaller in diameter than themselves. The white globules, on the other hand, flow more slowly and with greater difficulty through the vessels. They drag along the external portions of the current, and are sometimes momentarily arrested, apparently adhering for a few seconds to the internal surface of the vessel. Whenever the current is obstructed or retarded in any manner, the white globules accumulate in the affected portion, and become more numerous there in proportion to the red.

“It is during the capillary circulation that the blood serves for the nutrition of the vascular organs. Its fluid portions slowly transude through the walls of the vessel and are absorbed by the tissues in such proportion as is requisite for their nourishment. The saline substances enter at once into the composition of the surrounding parts, generally without undergoing any change. The phosphate of lime, for example, is taken up in large quantity by the bones and cartilages, and in smaller quantity by the softer parts, while the chloride of sodium and potassium, the carbonates, sulphates, etc., are appropriated in the special proportions by the different tissues, according to the quantity necessary for their organization. The albuminose ingredients of the blood, on the other hand, are not only absorbed in a similar manner by the animal tissues, but at the same time are transformed by catalysis and converted into new materials characteristic of the different tissues. In this way are produced the musculin of the muscles, the ostien of the bones, the cartilagin of the cartilages, etc. It is probable that this transfor-

mation does not take place in the interior of the vessels themselves, but that the organic ingredients of the blood are absorbed by the tissues, and at the same moment converted into new materials by contact with their substance. The blood, in this way, furnishes directly or indirectly all the materials necessary for the nutrition of the body." (Dalton's Physiology, pages 295 to 298, inclusive.)

Rapidity of the Circulation.—"The rapidity with which the blood passes through the *entire round of the circulation* is a point of great interest, and one which has received a considerable share of attention. The results of such experiments as have been tried show that this rapidity is much greater than would have been anticipated. Herring, Poissenille, and Matleneci have all experimented on this subject in the following manner: A solution of ferrocyanide of potassium was injected into the right jugular vein of a horse at the same time that a ligature was placed upon the corresponding vein on the left side and an opening made in it above the ligature. The blood flowing from the left jugular vein was then received in separate vessels, which were changed every five seconds, and the contents afterward examined. It was thus found that the blood drawn from the first to the twentieth second contained no traces of ferrocyanide, but that which escaped from the vein at the end of from twenty to twenty-five seconds showed unmistakable evidence of the presence of the foreign salt. The ferrocyanide of potassium must therefore during this time, have passed from the point of injection to the right side of the heart, thence to the lungs and to the pulmonary circulation, returned to the heart, passed out again through the arteries to the capillary system of the head and neck, and thence have commenced its returning passage to the right side of the heart through the jugular vein."

"By extending these investigations to different animals, it was found that the duration of the circulatory movement varied, to some extent, with the size and species. In the larger quadrupeds, as a general rule, it was longer; in the smaller, the time required was less." (Page 302.)

The round of the circulation through the arterial, capillary, and venous systems being accomplished in so few seconds, how almost inappreciable must be the time required for the arterial blood to flow from the left ventricle of the heart to the face; and yet there are distinguished physiologists who endeavor to explain in some other manner than the heart's action, the healthful blush or hyperemia on a maiden's cheek.

This leads us to consider the connection of the nervous system with the circulation of the blood, and particularly its connection with the capillary circulation. Since the great discovery, by Sir Charles Bell and Magendie, of the division of the nerves into "motor" and "sensitive," arising by separate roots from distinct portions of the spinal column, the next great discovery is that made by M. Claude Bernard as to the "vasomotor" nerves, or rather the nerves which control the contractility or dilatibility of the arterics. Dr. Brown-Sequard and Dr. Augustus Waller have also confirmed the truth of the discovery by M. Bernard. Together with him these gentlemen have added much to our knowledge of the nervous system. I have not seen the works or essays of either of these gentlemen, except a course of lectures delivered by Dr. Brown-Sequard and published in a book at Philadelphia in 1860.

The sum of this discovery is thus condensed in Dalton's Physiology: "Another very striking fact concerning the sympathetic, relates to the changes produced by its division in the nutritive processes of the parts supplied by it. One of the most important and remarkable of these changes is *an elevation of temperature in the affected parts*. If the sympathetic nerve be divided on one side of the neck in the rabbit, cat, or dog, an elevation of temperature begins to be perceptible on the corresponding side of the head in a very short time. In the cat we have found a very sensible difference in the temperature between the two sides, at the end of ten minutes; and in the rabbit at the end of half an hour. A muscular congestion of the parts also takes place, which may be seen to great advantage in the ear of the rabbit when held up between the eye and the light. The

elevation of temperature in these cases is very perceptible to the touch, and may also be measured by the thermometer. Bernard has found it to reach 80° or 90° F. The elevation of temperature and congested state of the parts are sometimes found to be diminished by the next day, and afterward disappear rapidly."

Sometimes they continue much longer. Galvanization arrests the increased flow of blood into the parts which were under the control of the divided nerve.

Dr. Brown-Sequard says, in his book, page 142:

Section of the Nerve.	Galvanization of the Nerve.
1. Dilatation of blood-vessels.	1. Constriction of blood-vessels.
2. Afflux of blood.	2. Diminution of blood.
3. Increase of vital properties.	3. Decrease of vital properties.

Dr. John Chapman, of London, who, in his essays, adopts the views of Claude Bernard and Dr. Augustus Waller, thus presents the case:

"Now I have proved by numerous experiments that cold applied to the back not only exerts a sedative influence (a 'depolarizing' influence, according to Dr. Tood) on the spinal cord, but also on those nervous centers which preside over the blood-vessels in all parts of the body. The *modus operandi* of this influence on those centers and its effects may be thus stated: 1. It partially paralyzes them; 2. By means of the partial paralysis thus affected it lessens the nervous currents in the vasomotor nerves emerging from the ganglia or nerve-centers acted upon, and stimulating the muscular fibers surrounding the arteries influenced; 3. By thus lessening those currents, it lessens the contractile energy of muscular bands of the arteries to which those currents flow, and by doing so facilitates the dilatation of the arteries themselves; 4. By thus inducing the condition of easy dilatability in the arteries acted upon, it enables the blood, which flows in the direction of least resistance, to enter them in great volume and with greater rapidity than before."

"The conditions here enumerated are analogous to those first induced in 1851 by Prof. Claude Bernard, when he divided the cervical sympathetic nerve."

“Of course, the physiological conditions induced by heat applied to the back are precisely opposite to those already enumerated as induced by cold: 1. The temperature of the sympathetic ganglia being raised, the flow of blood to them becomes more copious, and consequently their functions become more energetic than before; 2. Their nervous influence passes in fuller and more powerful streams along the nerves emerging from them and ramifying over the blood-vessels which they control; 3. The muscular bands surrounding the vessels are stimulated by this increased nervous afflux to contract with more than their usual force and so to diminish proportionately the diameter of the vessels themselves; 4. The diameter of the vessels being thus lessened, the blood flows through them in less volume and with less rapidity than before; indeed, it is probable that while the nervous ganglia in question are made to emit their maximum of energy many of the terminal branches of the blood-vessels acted upon become completely closed.”

“This series of effects caused by heat applied to the back is identical with that induced in the head by galvanizing the cervical sympathetic.” (Essay on Sea-sickness.)

Dr. Chapman has based a new system of practice upon the principles above expressed, to which I shall have occasion to refer in another chapter. The above extracts are intended to show clearly and fully what is understood to be the discovery of Claude Bernard and others. I hope that the reader understands it. I regret to differ from so distinguished authorities. Let us examine carefully what it is they teach. It seems to be agreed that the vasomotor nerves preside over and control the vibratory movements of the arteries. As to whether the vasomotor nerves belong to the ganglionic or sympathetic nervous system or originate in the upper part of the spinal column and medulla oblongata, and only pass through the ganglions of the sympathetic system proper, to be bound up in the same sheath with nerves originating in the ganglions, does not appear to me to be fully determined. There is certainly a discrepancy and an apparent contradiction.

As my views on this subject are so entirely different from the authors to whom I have referred, it is not important that I should notice these seeming discrepancies. We are told that the division of the cervical sympathetic nerve, by destroying the nerve-current which causes the arteries to contract, allows them to dilate more freely, which is followed by an increased flow of blood. On the other hand, by galvanizing the divided nerve, thus supplying the place of the lost nerve-current, the arteries are made to contract, and thus to carry less blood.

Dr. Chapman teaches that ice applied to the spinal column, and over the ganglia of the sympathetic nerves, benumbs or paralyzes the nervous centers, producing an effect approximative to a division of the nerves; and that heat, on the other hand, by stimulating the nervous centers, produces an effect analogous to galvanization.

A theatrical manager once announced the play of Hamlet with the character of Hamlet left out. So would it be to-day with many distinguished physiologists as to the part the heart performs in the circulation of the blood. It seems to me that they would be glad if the heart was left out of consideration. It would be so much more scientific to account for the circulation of the blood, particularly in the capillary system, by microscopical and chemical explanations.

It would make my book too large to notice fully this and many other kindred subjects. We are told nothing as to the effect of the division of an important nerve upon the action of the heart, whether it increases or lessens its force; nor are we told any thing as to the effect upon the heart's action of galvanization. If the facts were observed, they were considered of too little importance to be recorded in the books.

The power of an artery to dilate or contract is an inseparable and coexistent power. In other words, the contractibility and dilatability of an artery are coexistent functions, as much so as the systole and diastole of the heart itself. Annul or destroy one of them, and the other ceases instantly, and forever. Benumb or paralyze to a limited extent the power to contract or the power

to dilate, and the other is equally affected. Destroy one, and you destroy both, and in that case the arteries would exert no more influence upon a torrent of blood floating through them, than does a leather hose-pipe upon the water floating through it from a fire-engine. No physiologist will deny the fact that the arteries carry more blood to a gland engaged in active secretion than when the gland is not secreting. The arteries are then being more dilated, and yet the nervous system in all its parts is intact.

During health all glands secrete more freely and copiously; and during health the vasomotor nerves exert the greatest influence upon the coats of the arteries, and upon secretion generally. During the orgasm of erectile organs the whole nervous system is exerting its maximum influence, and yet the arteries dilate to receive the unusual quantity of blood. The division of the nerve in the necks of animals produced a lesion, *dis-ease* in the parts which had been supplied by the divided nerve. The *dis-ease* or lesion in the parts affected become a focus of irritation, and upon the old maxim of *ubi irritatio ibi affluxus*, blood flowed more freely to it, as to a part which is inflamed. If *all* the nerves going to the parts in which are observed increased heat, and increased vascularity, had been, or could be, divided, then the arteries passing through those parts would be as so many leather tubes, having neither the power to dilate nor to contract. But all the nerves are not divided, nor could they be divided. The sympathetic and vasomotor nerves are intimately mixed up with and bound up together with all the other nerves; so that the arteries still have the power to dilate and to contract to a more limited extent than if a nerve had not been divided.

But it is said that galvanization causes the arteries to contract, and thereby lessens the quantity of blood which flows through them. I would say that galvanization interrupts the vital functions of the parts under consideration, and the more particularly so because they are already in an unnatural condition. That the arteries assist the heart in carrying the blood to distant parts of the body, and in returning it through the capillary and venous

systems back again to the heart, I believe that all physiologists agree.

This force or power in the arteries is evidenced by their vibratory movements, causing what we call the pulse or pulsations. It is the coexistent power of the arteries to contract and dilate. When it is exerted to its maximum we have hyperemia of those vessels; venous congestion, on the other hand, depending upon weakened action of the heart. That this vibratory power of the arteries—that is, the power to contract and to dilate—depends on the vasomotor nerves, I have no doubt, and I will explain it by my theory.

I only ask physiologists to give the facts which I will present a full and candid consideration. I ask them, also, not to allow their minds to be over-influenced by authorities.

In the *Eclectic Journal of Medicine*, published by John Bell, M.D., Philadelphia, vol. 3, page 375, for the year 1839, will be found the following:

“DR. DARBY’S CASE OF HEMIPLEGIA.

“LEXINGTON, KY., May 10, 1839.

“DR. JOHN BELL:

“*Sir*—Being a subscriber to your journal, I send you a case for publication if you think it worth a place. Mr. R. aged about fifty years, during the course of a severe pneumonia, from which he was recovering, upon the occasion of a violent mental excitement was struck with a palsy of one side including the tongue. Mr. R. had felt for many years a numbness in the right arm; had been injured on the frontal bone during youth, from which, however, he soon recovered, and never experienced afterward any inconvenience. Was of a robust make, sanguineous temperament, rendered more excitable by free living. He died in ten days, in spite of the efforts of Dr. J. M. Bush and myself. No examination of the body was allowed, yet the case may be worthy of note for the following reasons: The right side was deprived of muscular power. On this side there was a strong, full pulse, while on the side not enervated there was no pulse at all. There was no difference of temperature on the two sides. The patient could not speak, but was entirely rational to his death. In reference to the pulse I will further add, that while I could scarcely feel it on the left side, it was so strong and full on the right side as to

authorize me to bleed him copiously about the second day of the palsy, and as I thought, with benefit."

"[As no post-mortem examination was made, any speculation on the cause of the phenomena in this case would be of little value. We omit those offered by Dr. Darby.—EDITOR SELECT MEDICAL LIBRARY.]"

The speculations which Dr. Bell declined to publish, because no post-mortem examination had been made, were given to Dr. Bush at the time. He was then Professor of Anatomy in Transylvania University. The same explanations have been given to many physicians and other persons in conversations on this subject.

Since the discovery of Sir Charles Bell, all physiologists, I believe, agree that hemiplegia, or paralysis of one side of the body, depends upon a lesion of the brain or medulla oblongata on the opposite side. The thing to be explained is the strong, full pulse on the paralyzed side, while there was at first no pulse to be felt on the left side in the radial artery, and afterward a very feeble one. The case reported in Bell's Journal was treated in the latter part of April or first of May, 1838. Since then I have treated many cases of paralysis more or less complete; having treated from the beginning of autumn in one year to the end of spring in another as many as nine cases. In every one of these I observed that the pulse was stronger and fuller on the paralyzed side than on the well side. In May, 1870, I was called to see a gentleman over eighty years of age. He had been suddenly attacked while sitting up in a chair to be shaved, but had been placed back in bed. On account of a fractured or dislocated hip-joint, and other broken bones, he had to be taken out of bed by several persons, and put back again in the same way. It being difficult to make out a diagnosis, for I could not tell whether he could move his limbs or not, on account of his old injuries, I was at a loss how to make out the case until I felt the pulse in both wrists. I immediately observed that it was much stronger on one side than it was on the other, and pronounced it a lesion of the brain and spinal column.

The paralysis afterward became evident, and the patient died on the eighth day.

I have called the attention of many physicians at the bedside to this difference of the pulse on the right and left sides, but have never seen it noticed in a book. My explanation of the phenomenon is this: The sympathetic system is a distinct one from that of the sensitive and motor nerves. It presides over the functions of organic life, and, as such, controls the vibratory movements of the arteries. By means of the nervous currents from the ganglions of this system, the arteries are enabled to contract and to dilate, and thus assist the heart in carrying on the circulation of the blood. The ganglions of the sympathetic system stand in the same relation to the functions of organic life that the brain, medulla oblongata, and spinal column stand in relation to the nerves of the special senses and to the nerves of sensation and motion. The ganglions of the sympathetic system being the centers of that system, do not originate in the brain, medulla oblongata, or spinal column, but send branches to those several centers of animal life, which branches have their roots or origin in the ganglions of the sympathetic system. The system of animal life and of organic life are so intimately connected together by these branches that a lesion of the brain, medulla oblongata, or spinal column, on one side of the body, involves and carries with it a lesion or paralysis of the system of organic life on the same side.

Hence it results, as in the case reported in Bell's Journal, that a lesion of the spinal column on the left side, causes them to lose the power to contract and to dilate. The arteries, having lost this power, become approximatively, according to the severity or extent of the lesion, like so many leather tubes, and consequently they are unable to receive and carry on the usual current of the circulation. Hence we have a smaller and weaker pulse on the well side of the paralyzed man; but it can only be called the well side so far as motion and sensation, functions of the animal life, are concerned. It is really the paralyzed side, so far as the functions of organic life are concerned. But there

being no lesion in the brain, medulla oblongata, or spinal column on the right side of the case reported, as in all other similar cases, the ganglions, or nervous centers of the sympathetic system remain, in a natural and healthy condition, arteries which are controlled by nerves coming from them retain their full power to contract and dilate, and thus carry on freely the circulation of the blood.

This is the explanation which I gave to Dr. James M. Bush at the time, and which would have been known to the world as early as 1839 if Dr. Bell had not declined to publish it. My then convictions have since been confirmed. In the nervous system of animal life there are nerves of motion and sensation, besides the nerves of special functions. In the nervous system of organic life I believe there are also two distinct sets of nerves—one which presides over the organic movements of the blood-vessels, now called the vasomotor nerves; and the other which presides over the secretions. Both of these sets of nerves take their roots, as I think, in the ganglions of the sympathetic system. It will be observed in the case reported that the action of the heart continued to be vigorous so as to justify, as I thought, blood-letting. The heart is an organ of animal life, and while it depends in part upon the nerves of organic life, it depends, to a greater extent, upon other nerves for its vital force.

The arteries on the left side of the case reported in Bell's Journal being paralyzed, and being approximatively like leather tubes, were unable to receive the usual quantity of blood, and hence there was a fuller and freer current of blood in the arteries on the right side, which were in a healthy condition.

In 1874 I treated a case of paraplegia in company with my friend Dr. James M. Bush, from June 18th to July 10th. On the 11th of July he left Lexington and returned to his home in Michigan. The paraplegia was complete. The lesion of the spinal column seemed to commence about the middle of the dorsal vertebra. The action of the heart was free and the pulse was regular and full in both arms. The man perspired freely above the line of paralysis. His intellect was clear and he was cheerful.

He slept well. His appetite was good, and he digested his food without inconvenience. Below the line of paralysis he was well nigh a dead man. The discharge from his bladder and bowels were entirely without his knowledge. He had no more control over the motions of his limbs, and had no more sensation in them than if they had been logs of wood. There is this qualification to the last remark, under the influence of strychnine he sometimes observed nervous twitchings in his lower limbs. There was not the slightest perspiration below the line of paralysis. No pulsation in an artery could be felt any where below that line. I felt every where where I thought it was possible to feel one. Sloughing commenced early on the hips and limbs, which we found it impossible to arrest.

Dr. Bush and I advised this man to return to his home in Michigan, where he died in about a month afterward.

Now, according to the theory of M. Claude Bernard, there being an entire paralysis of the whole nervous system, both spinal and sympathetic, below the line of lesion, the arteries were in a state of the most complete dilatability, and there should have been a full flow of blood to the very toes. According to my theory, they had lost entirely and completely both the power to dilate and to contract, and were, even the largest of them, like so many leather tubes. That some blood flowed through them I do not doubt, but this was forced through them entirely by the power of the heart's action, this organ having remained, up to the time we last saw him, in a healthy condition. The case referred to as treated by Dr. Bush and myself in July of 1874 was the only case of complete paraplegia I ever examined. I think it confirms, in the strongest possible manner, the truth of my theory, viz. that the complete paralysis of the ganglionic or sympathetic system of nerves arrests the vibratory movements in the arteries, thus limiting the passage of blood through them, and also arrests secretion. According to the theory of M. Bernard and his followers, the passage of blood through the arteries of the lower limbs in this case should have been freer and fuller; and I should think that the perspiratory secretions ought not to have

been arrested. If my theory is true his is exploded. The fullness of the pulse in one arm, and its feebleness in the other arm of a paralyzed individual, depends upon the severity or completeness of the paralysis—that is, of the lesion in the nervous system; at least this has been my observation. In these cases there may be said to be hyperemia of blood on the one side, and anemia of blood in the arteries of the other side, the hyperemia depending upon a vigorous condition of the heart's action, with full contracting and dilating power in the arteries within which the hyperemia exists; the anemia, on the other hand, depending upon the fact that the arteries in which there is but little blood have lost their power both to dilate and to contract, and are like unto leather tubes. In the case of paraplegia the arrest both of the circulation and of secretion caused the sloughing on the limbs which we found it impossible to control. The sympathetic nerves being completely paralyzed, the functions of organic life ceased. I will have to recur again to the subject of hyperemia and congestion and to the theory of M. Bernard when I present what I have to say on cholera.

The opinions—the demonstrations, scientists would call them—of Claude Bernard and his disciples may be satisfactory to microscopic observers and to analytical philosophers, but they would not be satisfactory to a Bichat or a Buckle; and perhaps mine would not be any more so.

As electrical forces are known to be active in the torpedo, or cramp-fish, I will give my views as to how they may influence the heart's action and the circulation of the blood in man. They were published in the St. Louis Medical and Surgical Journal of July, 1879, from which they are copied.

“A NEW THEORY AS TO THE FORCES THAT DETERMINE THE CIRCULATION OF THE BLOOD. BY J. C. DARBY, M. D., OF MT. STERLING, KENTUCKY.

“OLYMPIAN SPRINGS, KY., August 3, 1878.

“TO PROF. DANIEL VAUGHAN, M. D., *Cincinnati, Ohio*:

“*Dear Sir*—I have conceived a new theory as to the forces that determine the circulation of the blood. I send it to you.

“Positive and negative electricity are two forces equally efficient.

Bodies positively electrified repel each other, as do bodies negatively electrified; but bodies positively electrified attract those which are negatively electrified, and *vice versa*. I assume that in the animal body the left side of the heart and the arteries are positively electrified, and that the right side of the heart and the veins are negatively electrified. I assume also that the arterial blood is positively electrified, and that the venous blood is negatively electrified.

“I assume also that the arterial blood is kept in a state of positive electricity by its continual union with the oxygen of the atmosphere as the blood passes through the lungs.

“As I have to begin at some one point in the round of circulation, I start from the right side of the heart.

“The venous blood in the right ventricle being in a state of negative electricity, and the right ventricle itself being in the same electrical condition, they mutually repel each other. The pulmonary artery being positively electrified, attracts the blood in the right ventricle, which is negatively electrified. This constant repulsion between the venous blood and the walls of the right ventricle, and the attraction between the blood and the pulmonary artery, cause a continual stream of blood into the lungs, and explain why an artery conveys venous blood into the lungs. When this blood gets into the lungs, the electrical condition is changed, and it becomes positively electrified by its union with the oxygen of the atmosphere. It is then in an electrical state, to be attracted by the pulmonary veins, which, like all other veins, are negatively electrified. This explains why arterial blood is conveyed to the left side of the heart by veins, and not by arteries. When this arterial blood, in a state of positive electricity, enters into the left side of the heart, it finds that organ also in a state of positive electricity. In perfect health both the arterial blood and the left side of the heart are positively electrified to a high degree, and the repulsion between them is sufficient to propel the blood to every part of the body; the arteries also being positively electrified, continue this repulsion to their utmost extremities. As the blood passes from the arteries into the capillary system it parts with its oxygen and becomes negatively electrified, the reverse of what took place in the lungs. The blood thus changed in its electrical condition is forced into the veins, which are in the same electrical state that it, the blood, is. This is the only point in my theory which is not entirely satisfactory to me; but after the blood enters the veins the propelling force is clearly apparent; that is, the venous blood and the veins being in the same electrical condition, repel each other, and thus the blood is forced to the right side of the heart.

“The activity and healthfulness of the circulation depend very much upon the purity and quantity of the oxygen the blood receives as it passes through the lungs. I believe that it is admitted that perfectly healthy atmospheric air contains a certain amount of ozone or electrified oxygen. Atmospheric air which is not perfectly healthful contains a less amount of ozone. The regularity and healthfulness, then, of the circulation depend upon the purity of the atmosphere. Within an animal body, say man, I assume that the ganglia of the sympathetic nervous system are so many batteries generating electricity—whether positive or negative, has yet to be determined; the spinal column and the medulla oblongata are batteries also generating either positive or negative electricity, I can not say which; but I assume that the sympathetic nerves or their ganglia generate one kind of electricity, and the spinal column and the medulla oblongata generate the other kind. What part the cerebellum plays in this electrical apparatus has also yet to be determined. I do not suppose that the brain proper or cerebrum generates either kind of electricity, but that it receives positive and negative electrical currents, as does the heart, the liver, or any other organ. If negative electricity is generated by the ganglia of the sympathetic nerves, then that system presides over the veins and the right side of the heart; if the spinal column and the medulla oblongata generate positive electricity, then that system presides over the arteries and the left side of the heart. Now, my object in sending this to you is to have you test the truth of my theory by experiment.

“Your thorough knowledge of every thing connected with electrical forces and of all apparatus used in the generation of electricity will enable you, I think, to construct delicate bodies and put them in positive and negative electrical states, each one being separate. Then apply one of these bodies positively electrified to an exposed artery, and see if they will not repel each other; and then apply the same to an exposed vein, and see if they will not attract each other; and then repeat the experiment with a body negatively electrified. I would also want those delicately electrified bodies to be applied to a flowing current of arterial blood, which is positive; and to a flowing current of venous blood, which is negative. I have not the knowledge of electrical forces and of the apparatus used in their generation to determine these questions myself, and, if I had, my deficient eyesight would not permit me to do it. I sincerely hope you may be willing to make an effort to test the truth of my theory, and if you do, I know you will soon determine whether it is the discovery of a great physiological truth or merely the dreamings of my imagination. If

you can get access to a slaughter-house you can readily make experiments on exposed arteries and veins and also on flowing currents of venous and arterial blood.

“Very truly your friend, “J. C. DARBY.”

“CINCINNATI, O., October 13, 1878.

“TO J. C. DARBY, M.D., *Mt. Sterling, Ky.:*

“*Dear Sir*—Your letter of August 3d must have been delayed. I have not been able to give it as much consideration as it deserves, owing to ill health. In ascribing the circulation of the blood partly or wholly to the influence of electricity, your doctrine is somewhat analogous to one which I advanced in 1848 in regard to the movement of the sap in trees. It has been ascertained that the sap ascends by the wood of a tree to the leaves, and then descends by the bark and the cambium, which lies between the bark and the wood. In a pamphlet entitled *Vegetation Traced to Natural Causes*, I have shown that a circulation of galvanic currents in precisely the same course which the sap takes would result from the action of air and water at the roots and the action of light on the branches. To these galvanic currents I ascribe the movements and the elaboration of the sap, and subsequently found that indications of electric action were found by Becquerel, Bud, and others. The pamphlet alluded to was published in 1848, and was received very favorably by Dr. Yandell in the *Western Journal of Medicine and Surgery* for February, 1849.

“Although in a subsequent article I expressed some opinion of the probable part which electricity may act in the circulation of the blood, I did not pursue my inquiry far in that direction.

“Dr. J. W. Draper, however, about the same time expressed the same opinion, that the action of the heart is not capable of drawing the blood through the capillaries. The part which electricity plays in the human organization was soon afterward brought more prominently before the public by Alfred Smee, in his lectures on *Electro-Biology, or the Voltaic Mechanism of Man*”; his researches were mainly experimental, and they were afterward controverted by his opponents on the grounds that the living rabbits which he employed had their vital functions deranged by mutilation, and by the insertion of his needles for testing the galvanic currents, and it was accordingly contended that his results could not be considered normal.

“I have never seen Smee’s reply to his critics, nor am I aware that he made any; but since his death his views do not seem to have received much attention. His doctrines were first announced in a

lecture before the Royal Institute, and I think they have been published in the *London Lancet* for 1850, etc.

“In bringing your doctrines before the medical profession, it will be well to consider that the idea of the action of the heart is so deeply rooted in the scientific minds that it can not be easily eradicated. It may, on that account, be necessary to regard electricity as playing only a subordinate part, unless it can be shown that it produces the rhythmical movements of the heart. In taking the last view, there are some difficulties which it is not easy to remove. It is also necessary to consider that in experiments with statical or frictional electricity, very perfect insulation is necessary, and this does not exist in the organs of the human body. It was such considerations that induced me to regard the electricity concerned in vegetation as of a voltaic or dynamic character, and not analogous to that of the Leyden jar, or the electrical machines. Your theory might, perhaps, admit of a similar modification. No insulation is necessary for the successful operation of a galvanic battery. I would like much to be able to aid you as much as possible by making experiments to support your doctrine, but in my present feeble health it would be impossible for me to do any thing at the slaughter-house of this city. Besides this, it requires the most delicate and expensive electrical apparatus to detect the very feeble electric currents circulating along the tissues of plants or any part of the animal body. The idea of the analogy of the human frame and a galvanic battery is said to have originated with Napoleon Bonaparte, about the beginning of this century, on seeing one of the first batteries exhibited before *Academie des Sciences* at Paris. He pronounced it a resemblance of life.

“Your sincere friend, “DANIEL VAUGHAN.”

I have distinctly said, in the first chapter of this book, that dynamic forces are under the control of the laws of vitality. The theory above given is to be considered with this limitation. My distinguished friend Dr. Vaughan was correcting the proof-sheet of a scientific article on his death-bed, which article I suppose was published shortly after his decease. His letter in reply to mine is probably the last of his writings that have been published.

CHAPTER X.

THE RENEWAL OF LIFE.

We now come to consider briefly the disturbances which affect these several systems of animal life.

I can not better or more fully express the views entertained by scientific physicians, both in this country and Europe, when man is considered the subject of disease, than by giving a few extracts from the work of Dr. Thomas King Chambers, called *Renewal of Life*. He says, "M. De Vergie observed at the morgue in Paris, that for two months and a half after decease, the muscular structures still keep their natural forms and hues." "Up to three months and a half the scalp, eyelids, and nose so far retain their ordinary features that the age of the person may be told." Dr. Chambers goes on to say, "We may reckon with Drs. Bidder and Schmidt that the body of a mammal contains 35.45 grammes of nitrogen per kilogramme; and, therefore, that an animal of one hundred and thirty pounds (which is the mean weight of a man) contains upward of forty-six pounds of nitrogen."

"Then again, taking our numbers from an equally sound and independent source, we may reckon with Baron Liebig that the liquid and solid excreta of a man by kidneys and bowels for a year contain 16.41 pounds of nitrogen, or for three months and a half 4.7 pounds of nitrogen." That is to say, in three months and a half a quantity of nitrogen is removed by excretions, or vital decay, equal to the quantity of nitrogen in the whole mass of the chief nitrogenous tissue."

What attraction has this term of three months and a half for us? What memories does it rouse? Why, this was the very time we fixed upon for the fleshy framework of the corpse to melt away in. Here is a frequent fact, a light thrown on the mysteries of nature from a most unpromising source! Dead flesh and living flesh last as nearly as possible the same time—the former, if any thing, rather the longer.

As far as we can judge, the albumen, fibrin, gelatin, etc., which make up the living body, differ in nowise from the same matter dead; they are liable to the same changes; affected by the same reagents; and naturally are resolved into their element in the same time; just as the marble in the Apollo Belvidere is to a mineralogist the same stone that it was in the quarry, liable to the same accidents and possessed of the same properties, though temporarily endowed with a different value and made godlike by its adventitious form.

“What, then, raises to the rank of living creatures, and clothes with loveliness the masses of organic matter which are growing, moving, breathing, thinking, all around us? It is the power of individual life to create its own individual form. A man has no right of property over the particles of his body, except so long as they remain particles of his body and retain his shape. He hardly calls his the snippings of his hair or the parings of his nails, much less the carbonic acid he exhales from his lungs or skin; all that he throws off is by common consent claimed as a perquisite by the public; and the battle-fields which we have fertilized with his blood, enrich, not him, but the peaceful farmer. Yet as long as these organic constituents retain the form impressed upon them by the individual life, they are more truly his than any portion of his inheritance.”

“A conjectural theory has been hazarded, that life mysteriously endows living matter with a defensive virtue, which enables it to resist the chemical and other powers acting regularly on inorganic and dead matter. The most notable instance cited is the stomach, which, digesting every thing else, is not itself digested. This consumer of flesh is itself made of flesh, yet it is not con-

sumed. An answer seems given to the witty philosopher, who, on hearing an alchemist boast his discovery of a universal solvent, inquired, 'In what vessel do you keep it?' The stomach says (it had been in the habit of saying wise things even before the time of Menenius Agrippa), 'In a vessel like me, which is destroying, indeed, continuously, but is continually rebuilt.' Recent researches show that living matter, such as parts of living animals swallowed for instance, is dissolved by the gastric juice, and moreover, that its own epithelial coat is destroyed, but is immediately replaced by a new one. By this activity of growth (the idea of the impudent members calling the belly large!), and by a constantly flowing supply of alkaline blood to neutralize any of the acid secretion which might penetrate too deep, it retains the same shape for threescore years and ten. But it has no privileged immunity against the solvent it makes."

"It is then the form which constitutes the self; and it is not the changing, decaying matter which 'was mine, is his, and may be slave to thousands.' The organic materials are the property of the form only so long as it retains them, and no longer—they are a floating capital. Over the innate essential nature of the material it has no control. Life can not make the brute materials which it uses live longer than that which it leaves unused, but it has the power of making them anew, and building them up into a certain shape for the time they are made to last. In short, life rests on the metamorphosis or renewal of the body; as this renewal is more thorough, the individual is more perfect, and fulfills better and more completely the duties of its position. If it stops altogether, the body is no longer living. If it partially stops, the order of normal phenomena is deranged, and ease is expelled. There is a state which we call dis-order or dis-ease."

"To speak, therefore, of 'a superabundance of life,' or an 'excess of vital action, is a contradiction in terms. There can not be too active a metamorphosis of the tissues into their complete form, for the fresher their organic constituents the more serviceable they are, and the longer duration they have before them. There can not be too close an adherence to that typical

form which it is the business of metamorphosis to keep up, any more than there can be too exact an obedience to law and order."

"The most active metamorphosis of the body possible, the the highest development of life in every part, is HEALTH."

"The complete cessation of metamorphosis is DEATH."

"The partial cessation, or the exhibition of materials in an incomplete form, however copious they may be, is DISEASE."

"In death the flesh goes on being decomposed as during life; but not being renewed, the form is lost entirely. In disease decomposition goes on, but renewal flags, the incomplete tissues are retained as part of the imperfect body—a sort of 'death in life'—and are rightly termed by the pathologists 'degenerate.' They are generated but not regenerated; they are generated in an inferior mold of form."

The above extracts contain, as in a nutshell, the received scientific opinions of to-day as to the nature and philosophy of disease. They are based upon the physiological teaching to which I have so frequently referred in these pages, and they are very logical deductions from that teaching. I maintain that the theorem and the corollaries deduced from it are equally false and absurd.

There are laws which govern every thing, from the falling of the grains of sand on the bottom of the ocean, to the formation of the strata of rocks, which make up the crust of the earth; and from the growth of a blade of grass to the revolutions of the planets, suns, and stars in space. But these laws are separate, unlike, and distinct. Animal life is made up of four orders or divisions: radiates, mollusks, articulates, and vertebrates. But these do not differ more, the one from the other, than do the genera, family, or species into which they are subdivided.

When we come to man, the physiological and pathological life of a negro differs in many particulars from that of an Anglo-Saxon or Teuton. The same may be said of an American Indian, and some of the Asiatic races. But in all it is life. The laws which govern the charnel-house, which govern decay and rottenness, are peculiar to their own dominion. They may

invade the territory of vitality, but life retires as they advance. There is a very striking illustration of the permanency of the human organism in the mental faculty of memory. Many persons leave their homes and go to foreign countries, where they remain for a long time. After ten, twenty, or thirty years they return to the place of their birth, when a tree, the old paternal roof, or something, awakens memories which had been long forgotten, memories which would have never been recalled but for these awakenings. During this long interval the whole body, including the brain, had been entirely renewed, perhaps several hundred times, according to the theory of Dr. Chambers.

If you say that memory is a faculty of the mind, or of the soul—of something immaterial—then you must say that dogs, horses, and elephants, possess this immaterial something, call it mind or soul as you will, for they too possess the faculty of recollection after many years. I could give many interesting facts in proof of the memory of dogs, horses, and elephants. My explanation is, that all knowledge, however acquired, is photographed, so to express it, on the brain-substance, and that the brain-substance lasts as long as memory exists. It may be obscured—we call it forgotten; but incidents, like the rays of the sun's light, again will bring it to view, or make it perceptible to thought.

Destructive metamorphosis takes place when a man's body is losing weight, or the individual man is sick. But this disassimilation is under control of the vital law. Constructive metamorphosis or assimilation never operates in a living being while the body is losing weight.

The nitrogen referred to by Dr. Chambers was thrown off from the body, not by a destructive metamorphosis, as he supposes, but in the conversion of food into blood, as I have so fully explained.

The opinions expressed by the distinguished London physician, are made the basis of a false and very injurious medical practice, to which I shall hereafter more particularly allude.

In passing from a physiological to a pathological consideration of the secretions, of the excretions, and of the blood, I will

first notice the circulation of the blood under some of its phases, which, I think, are but little understood. I know that I have been able to do but little justice to the subjects about which I have written. But circumstances have prevented my doing better. It is the particular object of this book to treat of the secretions and excretions as the all-important matters to be considered in the treatment of disease. It is also my purpose not to defend, but to explain, what may be called *old-fashioned practice*. I am not defending any thing; I am overthrowing error; error which assumes to call itself scientific; error which is killing its thousands annually. It is my purpose to explain what functions the liver performs in a sick man, and how to regulate those functions. It is my purpose to explain the action of calomel, and to show its great value in the treatment of disease. I intend to show that Hippocrates, Galen, and Celsus, besides hundreds of other distinguished physicians coming down from their time, were not fools when they paid particular attention to the stools; that when they speak of bile—black bile, green bile, and yellow bile—they speak of realities which they saw, and not of suppositions which they imagined. I think I will be able to prove my positions to the satisfaction of every intelligent and candid thinker.

This leads me to the consideration of the subjects treated of in the next chapter.

CHAPTER XI.

EMETICS AND CATHARTICS.

As both emetics and cathartics, medicinally considered, have a special reference to the secretions and excretions; to the circulation of the blood, its congestion or hyperemia in different parts of the vascular system; to digestion proper; to the formation of effete matter and its removal from the body, I have written what appears in the foregoing pages.

No man will ever use either emetics or cathartics judiciously in the treatment of disease who does not clearly understand the questions I have discussed. I do not pretend to say that I have made them clear. No man can be more sensible than I am of the imperfect manner in which I have investigated these subjects. What I think I have accomplished is this: I think I have pointed out the true method of investigation. It is for younger and more able men than myself; it is for men who can see to read and write to follow the path which I have pointed out, as the woodsman blazes a tree here and there as he proceeds through a primeval forest. After him comes the horse-path, then the wagon-road, and next the railroad and telegraphic wires. It is for younger men to accomplish what I have been unable to do.

The compass of TRUTH has directed me.

I was born in Middlesex County, Virginia, within ten or twelve miles of the Chesapeake Bay. During my infancy my father removed to Richmond County, on the north side of the Rappahannock River, where I lived until I was twenty-two years old. This is the tide-water region of Virginia. On one or the

other side of the Rappahannock River there are marshes extending as far, I believe, as Fredericksburg. The same thing is true, I think, of all the rivers emptying into the Chesapeake Bay. This is the ague-and-fever and bilious-fever country of Virginia. These fevers have prevailed in that country every summer and autumn since Captain John Smith first landed at Jamestown. After I was old enough to remember, I had the ague and fever a part of nine successive summers or autumns. I remember well that I looked forward to the first frost with anticipated pleasure, for I had learned that these fevers ceased to prevail after a hard frost. I did not escape these fevers till I was old enough to be sent from home at a distance to school. Since then, whenever I have sojourned for a time in miasmatic districts I have had either ague and fever or bilious fever. It is reported of Celsus, the great Roman physician, that he, too, was born in a miasmatic district, and that he never recovered entirely from the effects of these fevers until he removed to a more healthy country. The white population in this region of Virginia are short-lived, very few reaching the age of seventy. The negroes, on the other hand, live to a good old age, many of them living to be very old.

The most common form of this miasmatic fever was ague and fever. I do not recollect of ever hearing of any one dying of ague and fever; though that form of disease seemed to weaken the constitution more than bilious fever, from which there appeared to be a more complete recovery. The method of treatment in both was to commence with an emetic. The usual time for giving an emetic was when the first indications of the chill or ague began to manifest themselves. The emetic medicines used were, for the whites a teaspoonful of ipecac and six grains of tartar emetic mixed in about eight ounces of warm water. A tablespoonful of this mixture was given to an adult every ten minutes until he began to vomit freely, when he was made to drink plentifully of warm water, until he had drunk a half gallon or more. To the negroes was given a different form of emetic—that is, to adult negroes. The same quantity of

tartar emetic and half an ounce of the sulphate of soda (Glauber's salts) were mixed in about eight ounces of warm water, and a tablespoonful of this given as in the other emetic, with the drinking of warm water, as in the other instance. This latter dose acted as an emetico-cathartic; and I well remember that the negroes got well in a shorter time than the white people. Both forms of emetics caused the liver to secrete bile freely, and to pour it out into the duodenum. I well remember vomiting bluish, greenish, and then yellow bile in great quantities, and I have seen many a patient do the same thing. In these instances the liver was certainly forced to act.

A great deal has been said in recent works on physiology about Pettenkofer's test for bile. There can certainly be no better test instrument for bile than a man's mouth, and I have tasted it too often to mistake the reality of its presence. The appearance of the bile vomited was exactly like that which passed from the bowels in my own case, and in that of hundreds of stools which I have examined as a physician. The stools to which I refer were those which followed calomel given in sufficient doses to excite the action of the liver. The effect of these emetics was first to arrest the chill or ague, and then to produce a copious perspiration. This was followed by a feeling of great comfort, and by a calm and refreshing sleep. After the emetics, and when the stomach had become entirely quiet, a dose of calomel was usually given. At the time to which I refer Peruvian barks were very generally in use. Quinine was high-priced, and very sparingly used. One or the other was given to anticipate the chill or ague. Many a nauseous dose of Peruvian bark have I taken.

Jalap and Peruvian barks were very frequently administered together, with an occasional dose of calomel. Under this treatment the recoveries were generally quick, and, unless there was some imprudence on the part of the patient, there were very few relapses.

I have said that the recoveries were, for the most part, quick; and that while living in Virginia I never knew of any one dying

from ague and fever. Then why was the white population short-lived, and why were the negroes long-lived? In answer to the latter, I can only here briefly state several facts. To answer the question fully would require me to go into a lengthy disquisition, which is apart from the subject I am considering.

It is a well-known fact, that in all miasmatic countries and in the tropics, negroes enjoy incomparably better health than do white people, especially that branch of the white race known as the Anglo-Saxon.

Du Chaillu relates that he found the negroes in equatorial Africa fully developed men and women with well formed limbs, sleek and fat, while *he* found it necessary to take tablespoonful doses of quinine every day to keep him from succumbing to the influence of the miasmatic poison. Although he lived through it, he became sallow and thin in flesh.

In answer to the second question, why the white race is short-lived in miasmatic regions, it will be necessary to go into a somewhat critical examination.

Dr. Daniel Vaughan published in the London, Edinburgh, and Dublin Philosophical Magazine, and also in several Cincinnati Journals, an article or articles on "The Chemical Nature of Malaria." Referring to the fact that an analysis of the air had failed to discover the cause of its insalubrity in miasmatic localities—that no connection could be traced between the prevalence of diseases and the complement of oxygen, nitrogen, or carbonic acid in the air, he advances the opinion that volatile oils disengaged from plants which are in a state of decay or decomposition under the influence of solar heat, are the efficient cause of all miasmatic fevers. He supposes that these volatile and ferment oils, when disengaged, arise into the atmosphere in the form of vapor, and in that form are received into the human system with the inspired air.

Dr. Vaughan shows clearly why this poison is most efficient or deleterious during the night, and why it also exerts a more injurious influence when the atmosphere is filled with moisture. Dr. Vaughan's valuable article will, no doubt, engage the atten-

tion of scientific and philosophic thinkers every where. It will not do to say that these volatile and ferment oils have not been detected in the atmosphere of the most deadly localities. Since the infinite divisibility of matter has come to be so clearly understood by scientific experimenters, indeed I might say, since it has been so demonstrably proved, it can not be denied that organic substances may float in the air in forms so inconceivably minute that they can not be detected by any test.

After these volatile and ferment oils, in a state of vapor, are received into the human body, Dr. Vaughan thinks, if I understand him correctly, that they were solidified or became resinous by oxidation; and that in that form they obstruct the minute blood-vessels. Whether this supposition is correct or not, I am not prepared to say. This much I know: some years ago I visited the water-cure establishment at Brattleboro, Vermont. I became well acquainted there with many gentlemen patients who had been old tobacco-smokers. They quit the pipe or cigar when they entered the institution. These gentlemen told me that after they had been packed in the wet sheets for several weeks, the sheets continued to smell of tobacco. It is not possible for a man to undergo a more complete sweating than is produced by water-cure packings; and yet the tobacco which had entered their systems, in the form of vaporized distillations, continued to remain in the blood and tissues of their bodies. I think there is the highest degree of probability of the truth in Dr. Vaughan's theory. If it be true, then the system may be supercharged with the volatile and ferment oils received into their bodies by respiration, when the individual so supercharged has one form or the other of remittent or intermittent fever. We know that Anglo-Saxons even, may become to a certain extent acclimated to a miasmatic country; that they raise families who become comparatively stout men and women, enjoy good health, that is to say, they become accustomed to miasmatic poison, and are enabled to tolerate it in their systems; yet being subjected to the influence of the same poison every summer and autumn, they finally succumb to it and are short-lived.

Up to the time when Dr. Elisha Bartlett published his valuable work on typhoid fever, the most apparent assignable cause for the existence of that fever in Paris, France, was *recent residence*. According to his investigations into medical reports and books written by the most distinguished physicians, this was the conclusion arrived at by them. Medical students from the country, and strangers sojourning in Paris for a time, were almost sure to have typhoid fever.

After reading *Les Miserables*, I immediately perceived that we could go one step further and show why recent residents were more liable to take this form of fever than persons who had lived in Paris many years. Victor Hugo gives a graphic account of the tons of accumulated animal and vegetable filth in the sewers of Paris. The cause of typhoid fever, then, in Paris, is a compound ferment oil, escaping into the atmosphere from the accumulated animal and vegetable filth existing in those sewers. This, I doubt not, is the cause of typhoid and typhus fevers every where. The old residents of the city have been accustomed to breathe it from their childhood and were less affected by it, while visitors were immediately poisoned by breathing these ferment oils diffused through the atmosphere. No doubt persons born there, both young and old, have typhoid fever. So it is also in miasmatic regions. Strangers from healthy countries visiting them when the malarious poison is most active, succumb to its influence, while many old residents enjoy good health. We thus see that while volatile and ferment oils escaping from plants produce intermittent, remittent, and yellow fevers, ferment oils escaping from decaying animal and vegetable matter combined, produce typhus and typhoid fevers. The first class of fevers being called, from time immemorial, idiopathic fevers, because no lesion is discovered; the second class having pathognomonic degenerations or lesions. We all know that smallpox can be produced by inoculation, but we also know equally well that if a healthy person, who has not been vaccinated, goes within thirty feet of an individual suffering under smallpox, in certain stages of that fever, he will take the disease and have it

with the same severity as if he had been inoculated with the smallpox matter. We see then that an individual can take smallpox by breathing volatile matter escaping from a smallpox patient. Now the amount of this volatile matter is so infinitesimally small that chemists have never been able to detect any difference in the air of a smallpox pest-house, to distinguish it from the air of the surrounding country.

In the case of this particular infectious poison breathed into the system, it requires, in almost every case, twelve days for it to begin to disturb the economy of the system. A very noteworthy instance of this is given by Dr. Thomas King Chambers, in his book already referred to. He relates a case of an individual recovering from typhoid fever who was accidentally exposed to a smallpox patient. The individual went on to improve daily and to increase in flesh up to the twelfth day, when the symptoms of smallpox began to manifest themselves, and the patient had a regular attack of smallpox.

As to miasmatic fevers, sometimes after a sojourn of a single night, as in the Pontine marshes near Rome, the poison is so concentrated and malignant that a fatal attack is immediate. But, as a general rule, the miasmatic poison, as well as the typhoid and typhus fever poisons, has to accumulate in the system, or requires time to effect certain changes in the system which terminate in a peculiar form of fever. There is, then, in each of the cases referred to, a certain form of poison received into the system, a cause which, being present, makes the disease; being changed, changes it; being taken away, removes the disease; more concisely expressed as follows: *Causa, quae præsens, morbum facit, mutata mutat, sublata tollit*. The question is, can we change or remove this *materies morbi*, this cause of disease?

Let us see.

Men may speculate as to the efficient or proximate cause of idiopathic or symptomatic fevers; but there can be no speculation as to the cause of smallpox. It can not be denied by any one that this form of exanthematous fevers is as malignant, as virulent, as fatal as any other form of fever. Sometimes smallpox

prevails under a mild type. During some seasons miasmatic and enteric fevers prevail under a mild form. If we can modify or lessen the severity of smallpox, we certainly can effect as much as to both miasmatic and enteric fevers.

Asiatics discovered, centuries ago, that smallpox could be made a comparatively mild form of fever. The method introduced from Turkey into England by Lady Mary Wortley Montagu was to prepare the system of the individual to be inoculated by dieting, by bathing, and by medicines, if necessary, and then to inoculate with the smallpox matter. The same thing is done to-day by the most successful surgeons in preparing their patients for operations.

I have as high an appreciation of the invaluable discovery of Dr. Jenner, as had the great Napoleon, who said to the Empress Josephine, "That man can be denied nothing." But vaccination is so carelessly and imperfectly performed that smallpox frequently prevails as a severe epidemic all over the United States. Some physicians believe, even now, that it would be better to inoculate after the method of the Asiatics. I do not think so, but what I have said clearly establishes the fact that smallpox may be made mild in form and character.

Proper dieting and cleansing of the body by washing with warm water are well enough, and all-important, but the most efficient means of preparing an individual who is liable to have, or who is certain to have smallpox, miasmatic or enteric fevers, is a thorough emetic.

Whatever may be the cause of the fevers just referred to, the first effect and the continued effect is to interrupt and prevent the conversion of food into blood. The function of every excreting and secreting gland in this complicated operation is disordered. Here arises within the body a new *materies morbi*, or cause of disease. Each of the fever poisons referred to, and the list may be extended to all forms of exanthematous fevers, as well as some other maladies, has the determinate power to fix the distinct and peculiar symptoms which follow, which symptoms are called pathognomonic, and entitle the fever to be called by this,

that, or another name. But they all agree in this, they interrupt and prevent the conversion of food into blood. They do this from the very beginning. They take away the appetite or make it capricious or unhealthy. The food is mixed in the mouth with the saliva, which is not healthful. In the stomach it does not meet with pure, healthful gastric juice. In the duodenum it meets with pancreatic juice equally deficient. All this, as I have clearly shown, is but the beginning of digestion or the conversion of food into blood.

When the fever poison is received into the body there is a struggle between the destructive force and the vital energies. This destructive force has a power within itself which determines its own progeny, and gives to that progeny a particular name; but it also has the power to impoverish and to vitiate the soil, so to express it, on which it feeds and in which it is developed. The more completely the natural, healthful functions of the body are disturbed and altered, the more completely the secretions and excretions are arrested or perverted, the more severe will be the fever in the individual case, call it smallpox, intermittent or remittent fevers, or enteric fevers. Now, in this struggle between the destructive and vital forces, the all-important thing for the physician to do is to maintain the latter.

In all forms of fever there is a feeling of indisposition of shorter or longer duration, but in every form, I believe, without exception, there is a sensation of chilliness, amounting sometimes to a distinct chill or ague. Now begins a manifest disturbance in the circulation of the blood. There is, during this stage, weakened action of the heart with its necessary consequence, congestion in the venous cavity. The sequences or consequences of this congestion, more or less continued, and recurring at intervals more or less regular, have already been referred to.

It was at the commencement of this stage, as I have already said, that emetics were given in the tide-water region of Virginia in the treatment of miasmatic fevers. It may be a question with some whether the fever poison can be removed or eliminated from the body. I do not doubt that it can be where the fever is

of miasmatic origin. Passing by my own experience and personal observations in that region I have, during many years' practice, succeeded in cutting short such fevers in two or three days.

The first thing I do is to give an emetic and vomit the patient thoroughly. It is a great mistake to suppose that emetics are only needed where there is an accumulation of food in the stomach. I will refer to that state of the case hereafter. Unless I have a reliable nurse I *give* emetics, and do not prescribe them. Every one who never witnessed the operation would be surprised to see the amount of thick, ropy mucus which is sometimes vomited up. By holding it up you will sometimes observe a cord a yard or more in length. Besides this viscid mucus, large quantities of bile are also vomited up. It would seem that the stomach possessed the morbid faculty which the lungs sometimes possess in chronic bronchitis—of separating or secreting this morbid mucus from its mucous membrane. By being thus thoroughly cleansed the stomach is put in a good condition for the reception of food which would have been disgusting before the stomach was cleansed. The liver being forced by the emetic to secrete and pour out bile, the circulation of the blood through that large gland becomes regular and active, and thus the congestion in the venous cavity is removed. This applies particularly to all the abdominal veins, and to the portal veins more particularly. The liver may be forced to act, partly by mechanical action by the movements of the diaphragm in the act of vomiting, when there is deep inspiration and full expiration, and by the compression of the abdominal muscles. These loads being removed, the action of the heart becomes freer and fuller, and the arteries bear healthful currents of blood to the remotest parts of the body. But besides this there is another function fully established, viz. the perspiratory functions of the skin.

When I give an emetic I am always careful to see that the bed-clothes are kept well around the body, and when they partly rise up to vomit I pull the bed-clothes up around their shoulders, and I am always positive in my directions that the bed-clothes shall be kept around the patient until the perspiratory action sub-

sides of itself. A sweet, quiet sleep commonly follows an emetic thus given. If the patient wants any plain, simple food, I allow him to have it. After this, if I think it necessary, I give calomel alone or in conjunction with other cathartics.

If it is a case of intermittent or remittent fever, I then give quinine or some preparation of Peruvian barks. The emetic has removed the first difficulties, the calomel or cathartics have given them an additional kick, and the preparation of Peruvian barks complete the cure. Sometimes, and very frequently, the remedies have each and all to be repeated. But I have had cases enough to establish the fact positively that even severe cases of miasmatic origin may be entirely relieved by one such round of medicines.

I saw a man whom I visited twice. He had had ague and fever for several months. I treated him as above stated. He told me a month afterward he had not been sick a day since. The quinine and cathartic pills I generally continue to give for some days. An emetic thus given always arrests the chill or ague. As soon as the patient begins to vomit freely, reaction of the heart commences, and you soon perceive a full, strong pulse. Animal heat, depending to a great extent on the force of the heart's action, upon increased motion in the current of the circulation, begins to be reëstablished in a healthful manner. The internal congestions are at the same time entirely removed, and a healthful though not excessive perspiration follows. The appetite is restored, and all the organs engaged in the conversion of food into blood are again at liberty and free to sustain this life-preserving function. How absurd it is to call an emetic thus given a depleting remedy! But physicians more frequently find it necessary to give remedies when the chill or cold stage has passed by. We have then to give them during the exacerbation or hot stage of the fever. You can not cut short a fever so quickly and surely when an emetic is given at this stage as when it is given at the beginning of the cold stage; but still, even then it is very effectual. It matters not what the form or the name of the fever may be, whether it be an exanthematous fever, as

smallpox or scarlet fever; or whether it be an idiopathic or enteric fever, emetics may be safely, advantageously, and curatively given in the early stage of each and every one of them. The heart's action being in this stage full and strong, the heat of the skin being excessive, the tongue for the most part dry and parched, and the thirst often very great, it will be found that an emetic properly given will remove each and every one of these difficulties, and there will again follow a moderate and healthful perspiration. A quiet and refreshing sleep will again follow this perspiration. It will also be observed that no headache or pain in the back and limbs follows an emetic given at the beginning of the cold or during the hot stage of the fever. If these pains and aches are present during the hot stage they will be removed, showing that the nervous system, in all of its parts, is relieved from the burden of disturbing causes, and is again able, healthfully, to perform its proper functions.

Now again the work goes on of converting food into blood in every part of the body. The forces of vitality are thus enabled to overcome the destructive forces, come from whatever quarter they may. In continued fevers of whatever name, as in remittent and intermittent fevers, emetics have sometimes to be frequently repeated. The well-informed and watchful physician must be the judge for the necessity of repeating them. When properly given they never weaken the system, and they are always followed by an improved appetite. The most dangerous and fatal form of all fevers is the congestive form. Emetics do more to break up and prevent this dangerous pathological condition than any other remedy. In fevers of miasmatic origin quinine should always be given after the emetic to prevent the recurrence of the cold stage. It will be found that the emetic prepares the stomach for the retention of quinine or some other form of Peruvian barks; and that these have a much more salutary effect after the stomach has been thoroughly cleansed, and after the circulation of the blood is healthfully reëstablished.

I have said that I am satisfied that fevers of miasmatic origin may be cut short or entirely removed in a few days. Can the

same thing be done in smallpox and enteric fevers? I admit that it can not; but yet I am entirely satisfied that the forces of organic or vegetable life will be better able to contend with the disturbances occasioned by the poisons occasioning these fevers now at work in the systems by the thorough working of a full emetic. It is unnecessary to repeat again what a full vomiting accomplishes. Physicians of all schools, I believe, agree that exanthematous and enteric fevers have to run a certain course. The length of that course has never yet been determined. It may be compared to a course around which race-horses run. Sometimes the track is smooth and easy; at other times it may be deep in mud, and it is difficult for the best horse to get through it at all. So it is in fevers. Now what the physician has to do is to keep the track in a good condition. If you remove the disturbances of the internal congestion on the one hand, or the exacerbations, with a burning skin and parching thirst, on the other, both and all of which entirely arrest the conversion of food into blood, you will make the track easy and the fever poison will pass over without disturbing much the individual who may be said to own the race-course.

Emetics and cathartics best prepare or enable each individual patient to receive and to convert the necessary modicum of food into blood while his body is the habitat of the fever poison. Even now a very large proportion of smallpox cases are fatal; and we know that many cases of typhoid fever continue for months before it can be said that the health of the individual is restored; and even then many are afflicted with weakened limbs or other infirmities. I am fully satisfied that a few emetics properly given at or near the commencement of these cases, would have saved thousands of lives from smallpox; and would have prevented the typhoid cases from being so protracted; with this additional and great advantage in both cases, that the entire restoration to good health would have been complete, without any hindrance or drawback. I have practiced medicine over forty-three years. I have not treated over a dozen cases of smallpox. One of them died; the first case I ever saw. The eruption was

confluent when I was called to see this case. I treat smallpox as I do scarlet fever, by vomiting the patient freely at the beginning of the malady. I will give one case. It was a complicated one. I was called to see a man of good constitution, fifty-four or fifty-five years of age, on the 23d of November, 1872. He was suffering intensely from acute pleuro-pneumonia on the right side. His respiration was both painful and difficult. His skin was very hot. His tongue was very dry and coated with a brown fur. He complained greatly of an intense pain in his head and back. His pulse was strong and full. Altogether the man was extremely ill. I gave him an emetic, he vomited freely, throwing the fluid from his stomach six or eight feet. After vomiting he breathed easily and felt comparatively comfortable.

I had a large, warm mush poultice put over this man's chest, which I directed to be kept warm during the night. I then gave him twenty grains of calomel. When I returned home, I told my family I believed I had been to see a case of smallpox. The man was so extremely ill that I would not alarm him by telling him that night that I thought he was taking smallpox. He slept comfortably a good part of the night. When I saw him about nine o'clock the next morning, I thought it advisable to repeat the emetic. The calomel had not yet acted.

After the emetic was over, and my patient appeared to be doing very well, breathing without difficulty and having a regular, full pulse, I then told him I thought he was taking smallpox. He did not think it possible, as he was vaccinated when a boy in England, and thought he had been exposed to the disease. The eruption commenced making its appearance on the evening of that day, and there was afterward a very full development of smallpox pustules over his whole body. After the second emetic I controlled the pneumonia with Norwood's tincture of veratria, given in doses of four drops every three or four hours, when the patient was not asleep.

This man was not uncomfortably sick after the second day. He ate corn-meal gruel and milk when he wanted it. The intervals for giving the veratria were lengthened until he ceased

taking it on the fourth day. The warm mush poultice was continued through the second day and night.

I believe if this man had been treated with placebos, or put on the do-nothing practice, he would have died, and that too in a very few days. As to feeding such a patient or compelling him to swallow milk, or beef tea, it was nonsense to talk about it. The only possible good it could have effected would have been to make him vomit, which would have been something in the right direction. His system was not in a condition to convert food into blood. I put it in that condition by vomiting him, and giving him the dose of calomel.

I consider the mush poultice and the veratria as adjuvants. The liver was made to secrete bile freely. It was passed from his bowels as thick as tar, and of a blackish and greenish color. I omitted to state that the calomel was followed on the second night by giving two five-grain pills composed of equal parts of calomel, rhubarb, ipecac, and aloes. These were continued for several nights, until the bile discharged was rhubarb yellow. I very seldom give calomel in exanthematous fevers, and only gave it to this patient because the pneumonia preceded the premonitory symptoms of smallpox. This man's system being well prepared, the smallpox poison had an easy course to run, and the patient was convalescent in a week.

I have treated a large number of what are called typhoid fever cases. I use the expression "what are called." In my opinion typhoid fever proper—*dothinerite* of the French—only prevails in this region of Kentucky as an occasional epidemic; and then it is for the most part confined to individual families, or particular neighborhoods. A physician in full practice will see every year, when there is no epidemic, one or more cases of *dothinerite*. But, for the most part, fevers prevailing here are of a mixed character, depending upon volatile and ferment oils which arise from both animal and vegetable matter in decomposition. A more correct name for such cases would be a typhoid form of miasmatic fever.

There is not, in such cases, so great a tendency to localize the

disease in the glands of the intestines. It is extremely difficult to make a distinctive diagnosis at the beginning in these cases; but I believe that in both it is best to begin with an emetic. If the case turns out to be only a typhoid form of bilious fever, its course may be limited to ten or twelve days, but if it turn out to be a case of *dothinerite*, the patient will be better prepared for bearing up against the poison originating it; as I have shown that it can be done in smallpox.

It has been so long since I read Andral's Clinique—before it was translated into English—that I can not be very positive, but my recollection is that one of the most marked symptoms in the typhoid fever cases reported by him was a greatly increased heat in the skin. I believe that Chomel and Louis state the same fact.

Now greatly increased heat of the skin is not a constant symptom in the cases called typhoid fever in this locality, but it frequently occurs. Increased heat of the skin is always associated with increased force of the heart's action. This increased force of the heart's action can not be constant. It is succeeded by shorter or longer intervals of weakened action of the heart. This weakened action of the heart always involves, as Dr. Cook has so clearly and fully shown, congestion in the internal veins having no valves. The thermometer frequently shows increased heat in the internal parts when the skin is not correspondingly hot. Shall I repeat again, that emetics properly given remove each and all of these difficulties.

In the treatment of fever of all forms, and particularly in fevers which have a more or less duration, whatever the physician expects to do he ought to do quickly. In enteric fevers especially, after a few days, or a week at farthest, almost every thing depends upon good nursing, with the observance of hygienic rules correctly understood. But a great deal is to be accomplished and can be accomplished during the first week. All that I pretend to do, or expect to be able to do, is to put the system of the patient in a condition to convert food into blood, and as far as possible to keep it in that condition. I have no doubt at

all of the medicinal value of cathartics, and especially of calomel, in the treatment of miasmatic fevers. I believe also that they may be used advantageously at the commencement of enteric fevers; but after a few days I never give them at all in such fevers.

There is another method of cure in the treatment of fever of all sorts, to which I can only refer in this connection.

I allude to cold water.

James Currie, M. D., F. R. S., of Liverpool, England, published in the latter part of the last century a book, which is known to the medical world as Currie's Medical Reports. I should have said was known, for the book is out of print, and there is not one physician out of a hundred who has ever heard of it. This work was written before the birth of Priessnitz, the author of the Water-cure Practice; but yet the practice revived by Dr. Currie, and so fully and philosophically presented to the medical world, had been forgotten when the hydropathic practice began to make a noise in the world, so that if a physician who had made himself acquainted with Dr. Currie's invaluable work attempted to use cold water in the treatment of fevers he was set down as a hydropathist, and so called even by physicians who ought to have been better informed as to the literature of their own profession. So great has been the prejudice against hydropathic practice that physicians who were afraid to say that their life is their own, because of their ignorance, would never have been induced to use cold water at all in the treatment of fevers but for the continual occurrence of a multitude of so-called accidents. I will give a single one. I could fill a larger volume than this with the details of similar occurrences.

A distinguished lawyer of this city, now deceased, told me that he had an attack of miasmatic fever many years ago at the house of a friend in Mississippi. He was treated by two or three physicians, who pronounced his case hopeless. They would not allow him to have a drop of cold water to drink, nor would they apply any to his person to cool his burning skin. One night his nurse went to sleep. He then crawled from his bed to a cool spring in

the yard, where he drank all he could drink. After this he lay down in the spring, which was a large one, and remained there till he felt comfortable. He then crawled back to his bed and was soon fast asleep. The next morning he was convalescent, but his ignorant and illiterate *scientific* physicians said it was a miracle he had not killed himself.

A reprint of Dr. Currie's book, with notes, would be worth dozens of the so-called scientific books which are presented to the medical public every year and sold at high prices. I do not confine myself to one remedy, which is too often the case in experimental practice in hospitals. In the treatment of fevers I use cold water, according to the instructions of Currie, in conjunction with emetics and cathartics. The three together, used at the proper time, and in the proper manner, will accomplish more than all the other remedies put together. They all unite and concur in one great desideratum, viz. to put the system in a condition to convert food into blood, and to keep it in that condition. As to the use of cold water in the treatment of fevers generally and the use of cold water and calomel in the treatment of typhoid fever in particular, the reader will find my views more fully expressed in the appendix of this book.

But I must now return to emetics and state some other forms of disease in which they are valuable. I will only add one word more about cold water. The drinking of cold water and ablutions with it are not at all incompatible with the use of calomel, provided the physician understands how to use both.

Having alluded to the Water-cure of Priessnitz, I have only to say, that, conducted as in the establishment at Brattleboro, Vermont, in 1847, I have no doubt that the practice is well suited to some forms of chronic disease; but a well-regulated system of dietetics has a great deal to do with the cures. As a system it is not at all suited to private practice; and in acute diseases the directions of Currie are far better than any ever given by an advocate of the hydropathic system; indeed, in a conversation with the gentlemanly and well-informed proprietor of the establishment at Brattleboro, Dr. Wesslehœft, about Dr.

Currie's book, he said he had read it, and added, that he considered it altogether superior to any work he had ever read on the use of water in the treatment of disease.

Hydropathy, as a system, has had its day. I have given one case of pneumonia, complicated with smallpox, in which I considered the emetics the most efficient remedy. There is no more successful method of cure in the treatment of acute pneumonia than an efficient emetic, to be repeated if necessary. I almost always follow it with calomel.

There is a notion now very generally received, that calomel promotes destructive metamorphosis. It is founded upon another error. Constructive metamorphosis ceases in all forms of acute disease, when the body is losing in weight. Destructive metamorphosis, or disassimilation is then alone at work. The capillary blood-vessels then remove from the solid organism certain materials which become effete matter. Calomel forces or induces the liver to separate a part of this effete matter from the blood and to convert it into bile, which has certain important duties to perform. The removal of this effete matter does not weaken the system; but indirectly prepares the way for constructive assimilation. There is no form of bodily and mental suffering in which emetics are more curative, than in that terrible malady called delirium tremens, or mania a potu.

I have treated a large number of such cases. Two of them died. One of them was a case to which I was called in consultation. He had taken morphine in large doses. The other was the case of a man who had had repeated attacks of this terrible malady.

I vomit these patients freely, and after their stomachs become quiet, give them calomel and jalap, if their bowels are costive, as they generally are, and calomel alone if they are not costive. A quiet sleep will generally follow this method of cure. The emetic has sometimes to be repeated; the cathartic always.

It will astonish any one who has never observed it, to notice the black and offensive stools which first pass off. I continue the cathartics until the stools become a rhubarb yellow. Under

this treatment the appetite very soon returns, and the digestive organs are again able to convert the food into healthy blood.

After the secretions are beginning to be healthfully established, if good broths, milk, and some other forms of food do not remove or quiet the nervous irritability, I then give some form of anodyne. Among the best of these is a strong toddy of whisky or brandy. In cases of threatened abortion I rely more upon emetics than upon any other remedy. Sometimes, and very frequently, the approximate cause of an abortion is undigested food in the stomach. Sometimes the symptoms of colic are so mixed up with the pains caused by the contraction of the uterus that it is difficult to make out a correct diagnosis. In such cases I always inquire as to the kind and quantity of food eaten and when eaten. I will give a case.

Over twenty years ago a gentleman said to me that his wife had had five successive abortions, that she was then in the fourth month of pregnancy, and he wanted me to try and prevent another mishap. This lady had five threatenings more or less alarming. I vomited her in every instance. At the fifth or last, near the eighth month, she was so alarmingly ill that I feared that I would not succeed this time. The mouth of the womb was dilated to the size of a silver half dollar, and the contractions of the womb were strong and frequent. I was called about the middle of the afternoon. I asked Mrs. P. what she had eaten for dinner. She said, nothing. I asked what for breakfast. She said, a little bread and butter and a cup of coffee. She said she had eaten about the same for supper the evening before. I then asked her what she had eaten for dinner the evening before. She said, a hearty meal of vegetables and meats. I gave her an emetic of mustard and salt. She vomited up an enormous quantity of sour and offensive vegetables and meats. She was immediately relieved and went to her full time, when she was delivered of a fine, healthy boy. She has had several since without any trouble. Could this woman have been relieved by any other method? I prefer the mustard and salt emetic in these cases, because it acts so quickly and does not relax or prostrate the system at all. I put a heaping tea-

spoonful of salt and one of ground mustard into eight ounces of warm water. This they swallow at a draught. I repeat the dose every five minutes until they begin to vomit freely. I then give the same quantity of warm water at a dose until the stomach is thoroughly cleansed.

Emetics are among the most valuable remedies in case of dysentery. In dysentery I would vomit with ipecac or lobelia. And this leads me to speak of the different kinds of emetic medicines. In cases of colic, depending upon undigested food remaining in the stomach, I prefer mustard and salt, as above stated. Where the bowels are costive, and I desire to cause the patient to vomit bile, I prefer tartar emetic. I have found tartar emetic particularly suited to the treatment of intermittent and remittent fevers. But it is said that in the states further South this form of emetic can not be safely given. It prostrates the system too much, and is too apt to run off on the bowels. Had I any such apprehension I would vomit with lobelia or with ipecac and lobelia combined.

Had Dr. Benjamin Rush or Dr. Nathaniel Chapman, of Philadelphia, or Dr. James Jackson, of Boston, discovered the emetic qualities of lobelia, and had they, or either of them, recommended it to the medical profession, it would have been considered one of the most valuable of the emetic medicines, but being introduced by an unknown man, and perhaps an illiterate man—I do not know—who at once commenced a war upon the regular profession, lobelia has always been associated with the steam-doctor system, and therefore discarded.

My motto being "*Aut viam inveniam, aut faciam*," I do not hesitate to try any thing when I have good reason for believing that it possesses some real merits. Sometimes, as just remarked above, it is not advisable to use the tartrate of antimony and potash as an emetic; and sometimes I have found that ipecac would not vomit. In such cases I use lobelia. It possesses this very desirable and in many cases all-important quality, that it does not excite the bowels, nor is there any nausea left after full vomiting.

But Dr. Samuel Thomson is entitled to far more credit for his method of giving an emetic than for the discovery of lobelia as an emetic medicine. I allude to the giving of a warm and stimulating tea at intervals of from five to ten minutes, say a teacupful at a time, until a quart is drunk, before the emetic medicine is given, which should then be repeated, as is usually done until full vomiting succeeds. I use the powder which the "Thomsonians" call composition powder to make this tea. It is composed, I believe, of ginger, capsicum, and bayberry leaves. This will prevent the gagging, straining, and cramping in the stomach. An emetic given in this way will be followed by a more abundant warm perspiration, which is a desirable thing. As I have once before remarked, the bed-clothes should always be kept well around the patient while vomiting, and should be so kept till the perspiration subsides of itself. Let who will try this mode of giving an emetic, I say to him that he will be pleased with it and also very much obliged to me for having the independence to recommend it.

From all that I can learn from books as to the nature of yellow fever, I have no doubt it is of miasmatic origin, and I do not doubt that a stimulating emetic of lobelia and ipecac, given as above directed at the commencement of the attack, would be one of the very best methods of cure. I have used emetics in many other forms of disease besides those named. But a man may have a large practice, and still only find it necessary to give an emetic occasionally. Ordinarily, in a majority of cases to which a physician is called, there is no necessity for prescribing so unpleasant a remedy. It is only in the severe forms of disease, and especially in epidemics which threaten life, that I find it necessary to use them. The taking of an emetic is a very disagreeable thing. It is for this reason that I attend to giving them myself, and do not prescribe them unless I have an attendant or nurse upon whom I can rely. Patients often say that they can not take them at all, that they strain, gag, and cramp them. I can assure them that an emetic given as I give it will do neither, and I have never been disappointed.

I commonly have half a gallon of warm tea prepared; of this I give three teacupfuls at intervals of five or ten minutes, and then put the emetic medicine into the remainder of the tea. Commonly I make them drink all of this, and sometimes more warm water besides. There is no remedy which leaves the patient feeling so comfortable. It breaks up at once many forms of disease, and prevents the long continuance of other forms which are wearisome, painful, and sometimes fatal.

During the past summer I was called to see a gentleman at one of the hotels who had just arrived from Cincinnati. It was 9 o'clock P. M. He left La Fayette, Ind., the day before. Being deprived of his dinner, he ate a lunch of hard-boiled eggs, bread, etc. This brought on cholera-morbus. He was a free drinker, and no doubt drank too much whisky. The night before a physician gave him chloral mixed with something else. It did him no good. He told me that the diarrhea was so continual that he passed nearly all the time between Cincinnati and this city in the water-closet of the railroad car. I found him nervous, restless, and irritable, with a quick, watery pulse. His stomach was nauseated. I gave him a stimulating ipecac and lobelia emetic. He vomited bile freely. After it was over he said, "Doctor, I feel like another man, I am entirely relieved." I gave him nothing else, and he slept comfortably all night. Next morning he ate a hearty breakfast of beefsteak, bread, and butter.

I repeat again that an emetic is a disagreeable remedy, but the forms of disease for which I prescribe it are far more disagreeable, with this great difference between them, that the taking of an emetic is of short duration, whereas the uncomfortableness and pains attending these same diseases are long continued, sometimes entailing, after apparent recovery, chronic ill health.

There is another form of severe malady in the treatment of which I have found emetics a most valuable remedy. I allude to spinal meningitis. That disease prevailed here last winter and spring. I treated six cases, four whites and two negroes. They all recovered. I do not say that my cases were as malignant a form of the malady as some others, many of whom died. While

treating them I read over, in conjunction with one of our most intelligent physicians, all that I could find in the books on the subject, particularly Andral, Flint, Watson, and the monograph of Dr. Clymer. Three of the cases had opisthotonus. The first case was a negro girl fourteen years old. She began to be delirious on Wednesday, the opisthotonus commenced on Thursday. I saw her for the first time on Sunday morning. The delirium and opisthotonus continued for ten days longer. I kept her nauseated a good part of the time with ipecac and lobelia, and gave her calomel at irregular intervals. At night I gave her a tincture of the root of aconite in doses of three drops mixed in a tablespoonful of water every hour until she went to sleep. It quieted her remarkably well. It would reduce the frequency of her pulse and make it fuller. I applied ice to the back of her neck, but did not see that it did her any good. This girl recovered with the loss of one eye. Physicians who can see tell me that the optic nerve is destroyed, otherwise she is perfectly well. It was several weeks before she regained her strength. Her diet was milk, beef, and mutton-broth, with Catawba wine after the acute symptoms had subsided.

CASE II. The mulatto man was twenty-five years of age, well-formed and athletic. He had had severe pains in the head for four or five days, and was seized the night before with a spasm, which lasted, as his wife told me, over two hours. This spasm returned next morning about daylight. I saw him about nine o'clock A. M. His limbs were terribly cramped. His hands were tightly clinched. His jaws were locked. His head was thrown back and his neck was rigid, in a bent condition. His pulse was forty. I attempted to open his mouth with a spoon. There was a little relaxation, when I gave him nearly a teaspoonful of calomel. One of our physicians asked me afterward why I gave him so much; I told him that I did not know that I would get another opportunity to give him any thing, and, that as many cases had died within twenty-four hours, I thought I had "better make hay while the sun shines." The calomel acted like a charm. Within six hours the tetanus and spasmodic

movements had all passed off. To remove the stiffness in his neck I afterward used twice the Georgia remedy. I applied to the back of his neck and between his shoulders a thick strip of flannel, saturated in spirits of turpentine, over this I rubbed a warm smoothing iron. It blistered the skin slightly and relieved the stiffness in his neck. I gave castor-oil to work off the calomel, which given in large doses is very slow to act on the bowels. This man's pulse continued at forty for three or four days. He was convalescent in a week.

In the case of the girl the temperature of the body was sometimes a little warmer than natural, in the case of the man there was not the slightest elevation of temperature.

CASE III. The first case of the whites was a lady some sixty-five years of age. There was no opisthotonus or delirium in this case, yet I considered it a case of spinal meningitis. She complained of intense pain in the head and back, with severe cramps in her knees, ankles, and feet. I gave her an emetic of ipecac and lobelia, and kept her on a nauseating mixture of the same afterward until the severe pain subsided. She also took pills composed of aloes, rhubarb, calomel, and ipecac. After she had begun to improve the pains in her knees, feet, and ankles continuing, and keeping her awake at night, I gave her twenty-grain doses of the hydrate of chloral. This lady had recovered but a few weeks before from smallpox. I treated her for that malady. She said her sufferings during the smallpox were nothing compared with what she underwent during this last sickness.

The next case, the old lady's grandchild, was a boy two years old. The child was extremely restless and apparently in pain. The eyes became crossed within a few hours after the child manifested any indisposition. I gave this child teaspoonful doses of a nauseating mixture of lobelia and ipecac. This was continued for ten or twelve hours. I then gave him enough of it to vomit him freely. He then became quiet. I then gave him two-drop doses of the tincture of aconite. He slept comparatively well during the night, having taken three or four doses of the aconite.

The next day, as the unpleasant symptoms were returning, I gave him two-grain doses of calomel every six hours. I also gave him an occasional dose of the nauseating mixture. The aconite was continued at night. The child soon got well. In neither of these cases was there any increase in the temperature of the skin, if there was any in the child it was very slight. His pulse was quick, the old lady's was not.

The next case was a young lady in her twenty-first year. She was well-formed and in the bloom of womanhood. She had been perfectly well up to the seizure. It commenced with intense pain in the head and back. In less than two hours she was delirious. There was no opisthotonus. I saw her at nine P. M., between two and three hours after the seizure. After a little trouble I succeeded in vomiting her freely. As this did not relieve the delirium, I gave her twenty grains of calomel. I then commenced giving her the tincture of aconite in three-drop doses. The second dose quieted her, and she slept four or five hours. When I saw this young lady early the next morning she was rational. She still complained of the pain in her head and back and annoying pains in her limbs. I repeated the emetic, and also the calomel, and afterward kept her on the nauseating solution of lobelia and ipecac. At night she took the aconite. Her nurse said this acted like a charm in quieting her. It was ten days before the soreness and pains in her limbs passed off; but she made a rapid recovery, and is now as well as she ever was. She was slightly salivated; the only one of these cases that was. There was not the slightest increase in the temperature of the skin.

The next case was a young lady about the same age as the last. There was slight opisthotonus, with severe pains in the head, back, and limbs. I vomited her twice, and gave her the cathartic pills above named. She also took the tincture of aconite. I applied a flannel, saturated with the tincture of turpentine, to her back and applied the warm smoothing iron. This was repeated twice. There was no elevation of temperature in her case. She recovered in a shorter time than either of the other cases.

It will be seen that I used chloral in one of these cases and the tincture of aconite in four of them. I relied upon the emetics, nauseating mixtures, and cathartics. The other remedies were simply adjuvants. The patients were all properly fed. Lobelia is to be preferred to any other form of emetic medicine in the treatment of spinal meningitis, because it produces relaxation more than any other article known as an emetic.

The late Dr. Benjamin W. Dudley, of this city, no less distinguished as a physician than he was as a lithotomist, used emetics in the treatment of chronic diseases to a greater extent than any other physician about whom I ever knew any thing. He effected many astonishing cures in cases which had been considered incurable. His emetic medicine was ipecac and the tartrate of potash and antimony. He commonly gave an emetic one evening and a dose of calomel the next night. This course he often continued for several months. He used the emetics and cathartics to effect this. Never perhaps was there a physician more careful and particular in his system of dietetics. He always prepared his patients for operations by dieting them and giving medicines after this method. I have very seldom used emetics in the treatment of chronic diseases, though I do not doubt the value and efficacy of them in many cases.

Emetics constitute a valuable remedy in the treatment of croup. Children can not be vomited like grown people. They will not drink either warm water or warm teas. Antimonial wine, wine of ipecac, tincture of lobelia, and syrup of squills, are each useful in cases of croup. Sometimes it is very difficult to vomit children having croup. We often have to try one medicine and then another. Sometimes I have found a mixture of molasses, not syrup, and common table-salt to vomit when other medicines had failed. I have vomited infants promptly with this, with very quick relief. I also give calomel in the treatment of croup. But after vomiting, if it can be effected, I have more confidence in cold water than in any thing else.

I fold up an ordinary size towel and wet it in ice-water. This I apply to the throat from ear to ear. It covers the throat as

low down as the sternum and clavicles. I could relate some remarkable cures, but it is unnecessary. A light fold of dry flannel is put over the towel.

To recapitulate: Emetics act in various ways. When undigested food remains in the stomach, causing colic or other uncomfortableness, an emetic removes it.

An emetic may be said to be one of Nature's methods of giving relief. Some of the lower animals, as dogs, vomit when they are sick. It is said that the Roman epicures vomited when they wanted to prepare themselves for the enjoyment of another meal. Many persons can vomit at will when they find that the food in the stomach is giving them trouble. But vomiting often occurs in a shorter or longer time after eating without any effort of the will; indeed these individuals can not prevent it. This involuntary vomiting often affords great relief; but it does not afford the complete relief which is given by medicinal vomiting.

This condition of the stomach is often the best indication for giving a regular and full emetic. It is one of the best means for quieting the nausea and retching, which often follows involuntary vomiting. The medicine selected must be suitable to the case.

Drunken men are often completely relieved by involuntary vomiting; and there is no better method to enable a man to get over a "spree" than to vomit him freely and fully.

As I have before said, there is no remedy equal to an emetic in the treatment of delirium tremens. But the cleansing of the stomach is but a small part of the benefit secured by an emetic properly given.

The lungs are continually throwing off a large amount of effete matter in the form of carbonic acid and watery vapor. When a man is well enough to work or to take exercise, he can cause the lungs to consume more oxygen and nitrogen—atmospheric air—by the muscular movements which compel his lungs to expand and to contract; but when the individual is unable to take exercise, then the only method by which you can compel the lungs to expand and contract is full emesis.

The mechanical movements involved, so to express it, effect this. But not only this, an emetic will cause the lungs to secrete and throw off mucus, as it does the stomach. An emetic regulates the circulation of the blood. It causes the action of the heart to become freer and fuller. It causes the blood to be distributed throughout every part of the body.

When the action of the heart is excited by a fever poison, and there is a strong, full pulse, the same thing occurs; but with this difference, the increased and accelerated current of blood caused by the fever poison causes or produces an increased amount of caloric, which torments and wastes the individual; whereas the accelerated circulation of the blood, caused by full emesis, does not produce an excessive amount of caloric, but is healthful and restorative. An emetic properly given produces, from the beginning, healthful perspiration, which prevents excessive heat, and which perspiration is another of the methods by which the matter, which ought to be removed, is thrown off from the body.

Emetics also force the liver to separate effete matter from the blood, and to pour it out into the duodenum in the form of bile.

Large quantities of bile are thrown off by vomiting. Sometimes much of it also passes off by the bowels.

These secretions or excretions being so fully established by an emetic, the kidneys will act healthfully.

In doing all this, an emetic necessarily acts upon the capillary system of blood-vessels, not only in the lungs, skin, liver, and kidneys, but in every part of the body. These capillaries may be said to be the mouths of the veins. They prepare whatever the veins have to swallow. There are two distinct sets of them. The one set is that system of vessels which takes up the food from the stomach and intestinal canal and empties it into the veins going to the liver. The other set is the system of capillaries existing in all other parts of the body. In perfect health the blood is changed in this system from arterial to venous. It loses oxygen and becomes filled with carbonic acid.

As I before said, when the body is fully grown, and there is

no increase in weight, nothing is added to the solid organism; but when the body is losing in weight, which almost always occurs in every form of sickness, then a part of the solid organism is removed in the form of effete matter.

The capillaries separate and remove this effete matter. It passes from them into the veins.

Now, the effect of an emetic acting through the lungs, skin, and liver, is to cause this effete matter so removed to be thrown out of the body. It causes the action of this system of capillaries to be more vigorous and healthful.

The coöperation of all of these health-producing efforts produces tranquility in the nervous system, and gives to it vigor and strength.

An emetic improves the appetite and enables the system to digest the food eaten; I mean to digest it according to my explanation of the meaning of that expression. A healthier and purer blood is sent to the brain and entire nervous system.

When these facts are fully and dispassionately considered, it will be difficult for a physician to overestimate the benefits conferred by this now very unfashionable remedy.

CHAPTER XII.

CATHARTICS.

If any one will read the latest essay on dew, he will find very little in it which was not observed by Aristotle. Passing from this, the greatest of Greek philosophers, the greatest perhaps because he took the whole earth for his field of observation, to Hippocrates and Galen, the greatest of the Greek, and to Celsus, the greatest of the Roman physicians, not to name many other distinguished physicians who lived in those remote ages, he will find that they too were close and careful observers of nature.

Aristotle was the Cuvier of his age. His pupil, Alexander the Great, gave him materials and opportunities for observation and investigation which no other man among the ancients had. The Greek and Roman physicians had no such advantages.

M. Auguste Comte, in one of his essays, maintained that sociology, or government, from the town corporation to the state, kingdom, or empire, was the most difficult department of human knowledge. Mr. J. Stuart Mill replied to Comte's essay. He reviewed the many and different departments of knowledge which a man must know and understand to become a physician, and declared that medical science was a more difficult department of knowledge to be clearly and fully understood than sociology.

If we agree with Mr. Mill, then we must admit that the early Greek and Roman physicians should stand among the first intellects of their age. Like the first astronomers, their opportunities for coming at correct conclusions were very limited. They described fevers in all of their stages with the greatest accuracy

and truthfulness, but the observations which they made, which come within the purview of this chapter, related particularly to the stools.

Natural philosophers now know something about dew, or give explanations about it which Aristotle could not give. He had neither a thermometer nor a barometer. So can physiological chemists explain as to the constituent elements of bile, but the Greek physicians knew bile when they saw it, as well as Aristotle knew dew when he saw it.

The bile which the Greek and Roman physicians saw, was that which passed from the bowels or was vomited. They describe it as yellow bile, green bile, and black bile. They noticed that the different kinds of bile followed the administration of certain medicines. They had neither a chemical apparatus to make an analysis nor Pettenkofer's method of testing it. But they were as certain that it was bile as Aristotle was certain that what he called dew was dew.

There are men living in England, as well as some in this country, who never get out of bed until the sun is two or three hours high.

If any such were living in Greece at the time of Aristotle, they might have said to him: "This thing which you are talking about, which you call dew, is an optical delusion; we want you to show it to us," but they would not change their habit of getting up late. So if Aristotle gathered some of the dew and brought it to them the next day, "Why this," they would say, "is water, and we know that it did not rain last night." Then Aristotle would take them out in the morning, after a shower, and show them the water on the grass and leaves, and say to them, "These drops, gentlemen, resemble dew." "But these," they would reply, "we know to be rain-drops!" Then Aristotle would say, "The dew evaporates under the influence of the sun's rays, under the influence of light and heat—it goes into the atmosphere." These skeptics would deny the correctness of Aristotle's observations because he did not prove them to their satisfaction. Modern physiologists, not knowing what the bile is made of, or what

becomes of it, deny that it is an excrementitious fluid, and teach that it is reabsorbed.

Until very recently no one ever doubted that the facts observed by the early Greek and Roman physicians as to bile were entirely correct and true. We are told that bile can not be detected in the feces by chemical analysis; that one of its most noticeable elements, cholesterin, is converted into stercorin, and passes off with the feces as stercorin.

We are told that in the body of a healthy man of ordinary size, two pounds and a half of bile are poured out every day into the duodenum. I ask physiologists what becomes of it, as the men asked Aristotle what became of the dew. They answer that it can not form a part of the feces, for two reasons: First, because it can not be detected in them by chemical analysis; and second, because there is too small an amount of the feces ordinarily discharged from the bowels of a man daily to account for the disappearance of so large an amount of bile. But when I then ask what becomes of the bile, they do not say that they do not know; but they say, whether changed or unchanged, it is absorbed.

Aristotle might have said that the dew went into the atmosphere; but these modern savans, having all the advantages of chemical and microscopical apparatus, can not tell what becomes of the bile. If it is absorbed by the capillaries of the intestines, it must enter into the portal veins; but no physiologist will pretend to say that bile, or any element of bile, can be found in the portal blood of a man perfectly healthy.

But let us look at this question of the continued absorption of bile, as a physiological fact in individuals entirely sound and well. They affirm that the liver separates from the blood two pounds and a half of bile every twenty-four hours. They admit that the gall-bladder is a receptacle, or sort of storehouse to the bile; but when it, or any of it, or all of it, is passed into the bowels it is to be immediately taken up again, absorbed, and sent back to the liver.

What need could there be for a gall-bladder if this conjecture is right? Why this continual chain-pump arrangement?

According to them, the bile has hardly had time to escape into the duodenum before it is taken up and brought back again to the liver.

But they tell us that the chief use of the bile is to assist the pancreatic juice in preparing the food for absorption. But according to them there is a great deal more bile poured into the duodenum than there is food emptied into it from the stomach, for very few persons consume two pounds and a half of food a day.

As is admitted by all, a large amount of the food eaten passes through the liver before it enters the general circulation. Now, according to this modern doctrine, more bile than food is being continually taken up and sent back to the liver.

The system of capillary blood-vessels, which absorbs the food in the intestines, possesses, according to these savans, the wonderful faculty of converting the bile into something so ethereal or spiritual that it can not be detected in the portal blood.

I wish the reader again to refer to the table showing the constituents of the bile. But these would-be learned men know no more as to what the bile is made out of than they do as to what becomes of it. They say that bile is not made out of effete matter, except cholesterin, a very small constituent part of the bile; and they deny that the liver forms or makes cholesterin. The effete matter they send to the kidneys. But they say the bile is made out of the blood. Then I would ask, what part of the blood is it made out of? Is it made out of the fibrin, the albumen, or the red and white corpuscles?

One would suppose that if two pounds and a half of these elements of the blood were consumed daily to be formed into bile, there would not be much left to supply the constructive assimilation, which they suppose to be continually going on. Two pounds and a half of the vital material is consumed daily, according to them, to form a fluid which is constantly being sent back to the liver. The whole thing is absurd. I will now again give my explanation of the bile from the beginning to the ending of it.

The flora and fauna, which inhabit this earth, agree in one thing, they live to eat and they eat to live. That is the sum total of vegetable and organic life.

Speaking of the animal kingdom, truly did Rasselas, a prince of Abyssinia, exclaim, "What," said he, "makes the difference between man and all the rest of the animal creation? Every beast that strays beside me has the same corporeal necessities with myself; he is hungry and crops the grass, he is thirsty and drinks the stream, his thirst and hunger are appeased, he is satisfied, and sleeps; he rises again and is hungry, he is again fed and is at rest."

After conception, after the birth of the spermatozoon in the utero—if I may so express it—all else is growth, depending on nutrition.

Commencing at the lowest level, we find living entities so simple in structure that it is difficult to say whether they belong to the animal or vegetable kingdom. Presently we perceive there is motion in them—motion evolving heat. They also possess the faculty to feed. The appetite of these simple beings seems to include an elective attraction, for their food may be said to come to them. This involves an absorbing system. Rising one step higher, we find that these simple existences have locomotion with the ability to move in a particular direction; and rising another step, we observe also that they have the faculty of sensation. These several faculties involve a brain and nervous system, yet the structure of these beings is so simple that it appears to be a homogeneous mass, the parts of which can not be discovered by the most powerful microscope. Yet these simple beings are perfect "after their kind."

Beginning at radiates, we find a development of animals perfect "after their kind" until we reach the highest of that order; and so on through mollusks, articulates, and vertebrates, until we arrive at man; and he too is developed from a spermatozoon, as simple in structure as was the organism of the little living entity which we could hardly distinguish from a plant.

The fully-formed and perfect organs which we find in man, as a bony skeleton, the heart, the lungs, the brain, etc., are but the

representatives of distinct individual beings of a lower order. These, taken together, make up the order, and the perfectly-formed organs in man make up the individual.

The different animals which make up an order, live an allotted time, unchanged in any thing which constituted their individuality. The organs in man remain for their allotted time, that is, during life, as unchanged and unchangeable as were these individual animals.

When we say that a man or woman is grown, we mean that all the organs are fully developed; as we speak of separate and distinct animals of another or lower order. It was a concurrence of parts which made each separate animal, and it is a concurrence of parts which make each organ.

The organs are as permanent as the individual animals. Men may waste from sickness or from starvation, and become living skeletons, as did Captain Riley in Africa, but that which they lost in weight was no necessary part of their individuality as animals. When a man, sound in body and mind, is grown, constructive assimilation ceases, and it is only renewed when there is loss in weight from any cause. By constructive assimilation I here mean that which developed to perfection each organ of his body. But a man may get fatter and his muscles may get larger. Here, too, we have constructive assimilation, but of another kind. It is the adding of something to the weight of the body which was no part of the original man. It is these unnecessary parts which are removed by destructive metamorphosis, but these two processes never go on at the same time. Destructive metamorphosis never begins until constructive assimilation ceases.

On the other hand, constructive assimilation never begins until destructive disassimilation ceases. Both of these functions are performed through the system of capillary blood-vessels.

These capillaries have no glandular or catalytic power. From the birth of the spermatozoon in utero, to full growth, each and every organ, or the nucleus of that organ (I refer to protoplasm as defined by C. Heitzmann, M. D., as given in a

former chapter) selects from the vital fluid floating in them, such materials as are necessary for its formation and growth. But the catalytic power is in the organ or the nucleus of an organ, bones selecting bone-matter, the brain selecting brain-matter, and so of the rest. But when destructive metamorphosis begins, then the capillaries take up from the wasting organism of each part whatever may be separated and thrown off. The capillaries do not separate compound substances, as urea and cholesterin, for instance. Such compounds are formed from the homogeneous and heterogeneous effete matter, by organs having glandular and catalytic powers, as the kidneys and liver.

I repeat, we find in the humbler forms of the lower orders, animals whose organism is so mixed and undefined that we can not discover in them any organs, and hardly the representation of a nucleus of an organ; yet we discover in them a capacity to feed, involving an absorbent system. We discover in them motion and heat, involving a respiratory system, for there is no heat without the consumption of oxygen; we discover in them sensation, with the power to move in a particular direction, which involves sensitive and motor nerves, with a brain to direct. Now I maintain that the development of perfect organs in man is but the representation of distinct living beings of a higher and higher class among the lower orders, and that they are as permanent and enduring in their forms.

Take the heart of man, for instance. It is nourished or fed through a system of capillary vessels. It has within it motion and heat. It is the seat of the emotions, and may be made to flutter by a look; the force of its beat may be, and is, continually influenced by a thought. Its action may be stopped at once, and sometimes forever, by what we see or hear. In all that constitutes life, it is a more perfectly developed organ than are many of the lower creations perfectly developed animals.

After full growth—I am here considering a perfectly healthy individual from conception to maturity—I maintain that if the individual does not vary in weight, neither constructive nor

destructive assimilation takes place in his body. What then, I may be asked, becomes of the food, and what are its uses?

If any physiologist will make the calculation, if he will weigh exactly all the food and drinks swallowed, and then weigh all the excretions thrown off from the body by the lungs, by the skin, by the bowels, and by the kidneys, he will find that they very nearly balance. This thing was attempted by Mr. Grisenthwaite, to whom I have referred, but there are so many errors in his book that I do not think it is entitled to much consideration. I suppose, however, the thing might be done with approximate accuracy.

In a living animal spirit and matter are inseparable. God created all living beings. In this creation he endowed matter with life. Life or spirit causes the development of organs and of animals. This is spirit acting on matter. In what we call nutrition, or the giving of physical or intellectual strength to man, may not vitalized matter or food give spirit, physical and intellectual strength to the several organs making up this totality without losing any of its material part? The union of the oxygen with the fresh juice of the grape, or with the malt of beer or ale, or the mash of corn meal and water in making whisky, gives strength, body, force, stimulating and intoxicating qualities without adding any material to the substance of either; so the force of vitalized matter upon the organized tissues—tissues according to Bichat. I think it may, and I think it does. This, I say, is the condition of things in perfect health, where the weight of the body remains the same from day to day.

The food is consumed in giving this spirit, which is manifested in animal heat, in organic movements, in the circulation of the blood, in respiration, in muscular power, and in thought. The organs by which the food is enabled to accomplish all this are called glands, with the accessories which assist the glands in their work, as the mouth, the stomach, the intestines, etc. In the condition supposed, the food passes through the body without adding one grain to its organized tissue, and without taking one grain from it in the form of effete matter. But this condition

does not often exist. In the variableness of human health there is continual loss and gain in the weight of the body. What I maintain of that condition is this, that loss and gain never go on at the same time.

We all know that individuals grow fatter and larger, and so increase in weight; but that increase of weight is but an addition of so much matter, which may again be removed without any alteration or change in the original forms or organs. This increase of weight is, of course, so much taken from the food, leaving less to supply the manifestations of power and force, to which I have alluded; and we know that most men lose in muscular and intellectual power as they become large and fat.

There can not be a shadow of a doubt that both the liver and the lungs effect important changes on the different kinds of food as it passes through them. The food is made to pass through these two glands before it enters the current of arterial blood; and we can conceive of no reason for this passage, except that some alteration or change has to be made in it.

The change effected in the liver I maintain to be this: The absorbents or capillary vessels which take up the food from the stomach and intestinal canal seem to have no choice or faculty of choosing; for they take up medicines and poisons as readily as they absorb the food. The liver acts upon all that is brought from the stomach and intestines; it separates something from it: what it separates is changed into bile. This is the first secretion of bile. It is separated from the food by the glandular or catalytic powers of the liver. This is the bile continually secreted, and afterward emptied into the duodenum in a state of perfect health. It is the bile which is secreted during growth from infancy to manhood, while constructive assimilation is active. It continues to be secreted and poured out into the intestinal canal as long as food is eaten and digested. It is the bile thus formed from the food, which commences to be poured out into the duodenum within a very short time after a healthy meal; and this kind of bile continues to be secreted and poured out as long as any food is passing through the liver.

This is the only rational explanation of the fact that the active excretion of bile begins soon after digestion commences and continues while that process lasts. But whenever destructive metamorphosis is at work, and effete matter is separated from the living organism and emptied into the veins by the capillaries, then the liver acts upon the effete matter and forms it into bile. Inasmuch as, while the glands are actively secreting, the venous blood returning from them is nearly as red as the arterial blood entering them, and as this is the case while the food is being absorbed from the stomach and intestines, I think it is evident that the bile-forming organs within the liver do not act upon effete matter while the food is passing through it; but that they act particularly upon it after the food has passed through the liver.

As said in a former chapter, comparative anatomy shows that the liver and lungs are compensatory or supplementary organs. They could not be compensatory did they not separate something from the food and blood, and unless the something separated was excrementitious matter, to be thrown off from the body.

I have said that the kidneys perform an important part in eliminating from the food elements or materials which they change into the liquid and solid constituents of the urine; that they act upon effete matter in certain pathological conditions, I also admit; but they act upon this effete matter as a heterogeneous mass, and form out of it urea and other substances.

I will not go over this again. The bile is an excretion, as the urine is an excretion.

We have now to consider the bile formed from effete matter. They deny the fact, and say that no bile is formed out of effete matter. One of the substances found in the bile, as cholesterin, they say, is found in other parts of the body, and therefore, according to their theory, it must be formed or made where it is found. Dr. Flint says, volume 3, page 268, "Cholesterin is now recognized as a normal constituent of various of the tissues and fluids of the body. Most authors state that it is found in the bile, blood, liver, nervous tissues, crystalline

lens, meconium, and fecal matter.⁶ We have found it in all these situations, with the exception of feces, where it does not exist normally, having been transformed into stercorin in its passage down the intestinal canal."

I do not deny the fact that cholesterin may have been found in the nervous tissues and in the crystalline lens; but I do deny that it was ever found in a nerve or in the crystalline lens of a perfectly healthy animal. Let physiologists look back and state from what subjects the nerves and crystalline lenses were taken in which they found cholesterin, and I will venture to guess that they will have to admit that the subjects were diseased ones.

It is formed in the liver, and whenever it is found in any other part of the body, excepting the bile, it is because it is carried along with the bile into the hepatic veins, and thus finds its way into the general circulation.

We know that in jaundice the bile does not all pass into the hepatic and cystic ducts, but that some of it escapes into the hepatic veins. A part of the bile may thus escape without being enough to cause discoloration of the skin and conjunctiva.

Being a compound principle, it may be lodged in the tissue of a nerve or in the crystalline lens, just as embolism occurs in blood-vessels. Any other supposition would be to suppose that the nerves and the crystalline lens have the glandular and catalytic power to form proximate principles or compound substances, which are no part of their healthy and perfect organism.

Cholesterin is formed, as is every other proximate principle found in the bile, by the glandular and catalytic power of the liver, and it is formed out of effete matter.

It is, no doubt, impossible to separate the bile into two parts, and to say that this part is formed out of the food and that part is formed out of effete matter; but the part formed out of effete matter can only be formed out of effete blood, or when destructive metamorphosis is going on, and the body is losing in weight

by a separation or throwing off of a part of its solid organism. This condition, as I have before said, never occurs when constructive assimilation is in active operation. No physiologist denies that effete matter, so defined, must be taken up by the capillaries, and that they empty it into the veins.

This compels me again to repeat that the capillary blood-vessels have no glandular or catalytic power.

The blood is created out of the food by glandular action. The excretions, for instance, those of the liver and kidneys, are created out of the food and effete matter.

Physiologists, having adopted the theory of constructive and destructive metamorphosis continually going on in animals in health as in sickness, are compelled to find work for these two processes to perform; and as cholesterin and urea are sometimes found in or near certain tissues, they are forced to conclude that these two compound substances, as well as all other similar ones, are produced in the organism or by the capillaries, which convey the blood to them.

This conclusion leaves those wonderfully elaborate organs, the liver and the kidneys, nothing to do but to remove compound substances, such as are found in the blood and bile, from the blood.

They can not say that all the other compound substances or proximate principles found in the bile, except cholesterin, are not removed in this way; for when they say the bile is certainly absorbed, and yet admit that they can not detect it in the portal blood, they are estopped from denying that it may exist in the blood, and that the liver only removes it as that organ removes cholesterin and as the kidneys remove urea. If cholesterin and urea were normal constituents of healthy organisms, as they maintain, we would have blood-poisoning from the excess of these compounds in the blood as a malady of constant occurrence; for any derangement of the two processes of constructive and destructive metamorphosis might occasion an excess of these deleterious principles, whereas uremic poisoning is fortunately a disease of rare occurrence.

I maintain that the liver forms or makes out of the food and out of effete matter cholesterin and every other principle found in the bile. I maintain also that the kidneys separate from the food urea and every other constituent found in the urine; and that all the urea that the kidneys ever take up from the blood is that which was first formed in them, and which passed into the renal veins, because, for some cause, it was not emptied into the ureters. I maintain that the kidneys do not act upon effete matter at all when the animal is in good health; and, if at any time they do act upon it, it is as a vicarious office—the liver, perhaps, not performing its duties in removing effete matter from the blood.

We know that the kidneys do act upon certain kinds of food, as asparagus and the juices of watermelons, because we can detect this by the odor, etc. Besides, there are many vegetable medical substances which act as diuretics. These may be considered as a part of the food. Moreover, there are many mineral salts, as the bromide, iodide, and acetate of potash, which pass out of the body directly through the kidneys.

Now all these medical substances are taken up from the stomach or intestinal canal along with the food, and pass with it, more or less directly, to the kidneys. By some law of nature which we can not understand, all of the diuretics, both vegetable and mineral, pass through the liver and lungs, as far as we can tell, untouched, and go directly on to the kidneys, by which organs they are removed.

I have said that the liver removes from the food certain parts of it, which it forms into bile. Is it unreasonable to suppose that other parts of the food, like the vegetable and mineral diuretics, go directly to the kidneys to be acted upon by them?

We know that in the disease called diabetes mellitus, when the liver fails to change the saccharine part of the food in its passage through it, all of this saccharine matter passes directly to the kidneys, and is removed by them.

This malady is very erroneously supposed to be a disease of the kidneys, whereas it depends entirely upon the liver failing

to perform one of its most important functions—viz. the conversion of every form of food containing sugar into something more akin to blood. I do not deny that after the malady has continued for some time the kidneys themselves may become diseased, because they have too much of this vicarious work to do.

It would be too much of a digression to say any thing more here concerning diabetes mellitus. I only refer to it to give another proof that the kidneys do act upon the food, and not upon effete matter. This heterogeneous substance is only separated from the living organism during sickness, or when the body is losing in weight from any cause. It is all emptied by the capillaries into the veins, and it is all carried, in my opinion, directly to the liver by a law of animal economy like unto that which carries diuretics to the kidneys. The bile, then, is formed from the food and from effete matter.

We have next to inquire what becomes of it after it is poured out into the intestines. The elaborate, voluminous, and learned works of Drs. Flint and Dalton may be said to contain all that is known of human physiology.

When I say that they teach, I, in effect, say that all physiologists teach that after the bile is poured out into the duodenum it is absorbed, and goes back again into the circulation. Hence it is, as they teach, a secretion and not an excretion, except the cholesterin. Hence, with that exception, it is not formed from or made out of effete matter. All physiologists agree that effete matter has to be thrown out of the body as an excretion by some channel or other; and as they deny that bile is formed out of effete matter, they are forced to find some other channel for the exit of this excrementitious substance. Thus it will be seen that an erroneous theory as to the origin of bile compels physiologists to select the kidneys as the organs by and through which effete matter is removed from the living body.

If I can show that bile is an excrementitious substance or fluid, then they must admit that it is formed from and out of effete matter. When I enter upon this field of investigation they must admit that I am standing upon *terra incognita* to them, for

having said that the bile is absorbed, they are precluded from any reflections or criticisms on what I may discover, or on what others may discover, or may have discovered.

In coming to the conclusion that the bile is absorbed, they give as a reason for this conclusion the fact that the bile can not be detected in the feces by chemical analysis. If the bile does not enter into the composition of the feces, then it does not pass out of the bowels through the rectum; and then also we are shut up to the unavoidable conclusion that the feces are composed entirely of that part of the food which is not absorbed during digestion, and of mucus and desquamated epithelial cells.

All observations made upon the stools before the discovery that the bile is absorbed were erroneous; and the observers, from Hippocrates to the present day, who say that they saw or have seen bile in the stools, were and are a set of nincompoops. I believe that Galileo was once supposed to belong to that class of mortals.

As the infant in utero swallows nothing, it has puzzled physiologists to tell what the meconium is formed out of; and as cholesterolin is found in it, they have another difficulty. We who say that the meconium, except a little mucus mixed with it, comes from the liver and is made out of effete matter, are of course nincompoops. Being such, and finding that the liver in the fetus and in infants is excessive in volume compared with the remainder of the body, we suppose that it is the organ which eliminates effete matter from the blood and keeps it in a pure condition; and we are strengthened in the position because we find also in the fetus that the kidneys performed little or no duties, there being very little if any urine in the bladder. Now if the kidneys alone remove effete matter from the blood, if urea is a constituent part of effete matter, being continually formed by constructive and destructive metamorphosis from the moment of conception, then we say that the fetus in every instance ought to die of uremic poison, because the urica must remain in the blood.

But if, as I maintain, the kidneys act exclusively upon the

food and form urea and other compounds out of it by the glandular and catalytic power of their wonderful organism, then urea will never be found in the blood of a healthy fetus or of any infant before it sucks.

If urea is at any time found in the urine of the fetus, it is because the mother is not in a healthy condition, and there is more or less urea floating in her blood.

If constructive and destructive metamorphosis are continually going on in the body of a healthy growing animal, then there must be, according to Dr. Dalton, over thirteen complete and entire changes, and according to Dr. Thomas King Chambers over six such changes during fetal life of two hundred and seventy days.

If such a law exists in the animal economy, it is a constant law, and operates as certainly during fetal life as it does in the development and growth of the tiniest radiate, mollusk, or articulate.

Now I ask what becomes of the effete matter so separated during fetal life? Any answer would involve an absurdity as manifest as the *reductio ad absurdum* in any geometrical problem. But if, as I maintain, destructive metamorphosis never operates during healthy growth, then we might expect to find what we do find—viz. a very small amount of meconium in the bowels of a new-born infant. This small amount may be produced, and no doubt is produced, by the variableness in the health of fetal bodies; which allows that law to act as it does in healthy grown people whose bodies occasionally change in weight. But it may also be produced by the liver acting upon blood which may be said to be effete. In either case the meconium is formed by the liver, and is the first form of bile.

To return again to the feces. If they are composed entirely of that part of the food which is not absorbed, and of mucus and desquamated epithelial cells, then it would be difficult, if not impossible, to account for the large amount of fecal matter which is discharged from the bowels by many individuals in health as in sickness; for we know that some kinds of food, as milk and

nutritious soups, not to name many other forms, are about all absorbed. During the sickness of many persons we often notice large fecal discharges when patients have eaten little or no food for several days.

If the liver pours out two pounds and one half of bile daily into the duodenum, we can easily account for the large amount of feces discharged during this abstinence from food.

This is upon the supposition, which I maintain to be a fact, that the bile enters into the composition of the feces and is formed, in sickness, out of effete matter. I know for a certainty that such discharges do occur during sickness when the patient eats little or no food; and I know also that such discharges follow the administration of medicines which excite the liver to the formation and pouring out of bile.

Physiologists say that cholesterin does not enter into the composition of the feces as cholesterin, but is changed into stercorin, and forms a part of the feces under that form. If the cholesterin may be changed in form, I ask how do they know that the other constituents of the bile may not also be changed so as to prevent them from being able to detect these constituents of the bile in the feces?

Going back, no doubt, to the days of Hippocrates, all medical observers have witnessed that healthy feces have free properties or qualities peculiar to them—viz. the odor, the color, and the consistency.

These qualities or properties are necessary parts of the composition of feces. Whenever the feces are deficient in any one of these qualities or properties, then they are recognized as unhealthy by all medical observers who think as I do. Of course we are supposed to be nincompoops by that highly respectable class of gentlemen who assume to be scientific physicians; but we number on our list a vast multitude, among whom are many names supposed to be immortal.

It is constantly observed that the feces are ashy or clay-colored, and the individual discharging such is dyspeptic or otherwise sick. If we give such a patient calomel or some other

medicine which acts upon the liver, we notice that the feces become dark in color, or more consistent, and have the odor peculiar to them. I say this is because the liver has been made to act. The same remark may be made to apply to a hundred forms of indisposition or ill health.

I have already alluded to the remarkable fact, that physiologists have not taken into account the very large amount of excrementitious effete matter which escapes from the bowels in the form of gas. If I assert that cholesterin passes off with the gases, who is prepared to deny that I am correct? The same thing may be true as to the disposition in part of other constituents of the bile. It is no doubt true as to the disposition of a large amount of fecal matter by many individuals. They are the persons who go for days, and sometimes for several weeks, without discharging fecal matter from the bowels. We know that in such cases when there is an operation or alvine discharge, it is in the form of hard balls called scybala. In these instances the fluid part of the feces has escaped with the gases.

Assuming, as I do, that constructive and destructive metamorphoses do not go on at the same time in a living animal, then it follows that the only source of bile, when constructive assimilation is active in the animal body, is the food passing through the liver.

In this condition of health the constant flow of a large amount of bile must be from the food. So far as humanity is concerned, there is no secretion and excretion on the regular and healthful performance of which *mens sana in corpore sano* so much depends as on the constant and uninterrupted secretion and pouring out of healthy bile.

Knowing, as we do, that a very large amount of food passes directly from the stomach and intestines to the liver, is it not, among all things, reasonable to suppose that the liver acts upon the food as it passes through it? And are we not strengthened in this conviction because we know that bile begins to be poured out freely immediately after the food begins to be absorbed? If I am correct, the liver separates from the food such parts as

ought to be separated, and pours them out in the form of bile into the duodenum. Bile derived from such a source must of course be excrementitious, and it has to pass out of the bowels with the gases which escape from them or as a component part of the feces. But if again destructive metamorphosis is working when constructive assimilation ceases to act, then the only source of bile will be the effete matter separated from the living organism.

If the individual under consideration takes some nourishment, of course the liver will separate, or attempt to separate, the form of bile above spoken of from it. This labor should not be thrown upon the liver and other glands during the continuance of maladies in which destructive metamorphosis is active.

There can not be any greater error in the practice of medicine than to compel patients, laboring under acute diseases, to swallow large quantities of food daily, under the notion that this food goes to supply the place in the living organism of the effete matter removed by destructive metamorphosis; or, as Dr. Chambers expresses it, "to put in a new brick where an old one has been taken out." Dr. Chambers admits that hospital patients do much better under this system of treatment than do patients in the higher circles of society.

It is, perhaps, because the food furnished in the hospitals is so much better, more nutritious and digestible than that which the hospital patients were accustomed to, that it acts as a healthful stimulus, enabling the liver and all other glands engaged in digestion to perform their work more easily. But this is a digression.

When destructive metamorphosis is in operation, very little food is ordinarily consumed, and a large amount of effete matter is continually thrown off from the body. This is evidenced by the daily loss of weight in individuals. Now, according to the teaching of all physiologists, this effete matter is removed from the blood by the kidneys. This assumption is in the face of the well-known fact that during many forms of sickness, in which there is great loss of weight, there is very little urine

poured out by the kidneys into the bladder; certainly not enough, nor any thing like enough, to account for the amount of effete matter separated from the living organism by destructive metamorphosis.

All physiologists and all physicians know, that unless the effete matter so separated is eliminated from the body it will aggravate the malady, leaving little or no chance for the patient to recover. Death from uremic poisoning is one of the results of the retention of one of the forms of effete matter in the blood.

The retention of any form of it is more or less deleterious. Now I maintain, as up to a very recent time all physiologists maintained, that the liver removes the effete matter from the blood; and I affirm, as they affirmed, that the well-being or improvement of a patient depends more upon the liver regularly removing the effete matter than upon any other one of the organic functions.

It is thus again that I show that bile is an excrementitious substance, which either passes out in combination with the feces or escapes from the bowels with the gases. The bile, then, being formed from the food and effete matter, it follows necessarily that if only a small quantity is eaten, but little bile can be formed from the food; and if the body is losing but little in weight, then only a small quantity of bile can be formed out of effete matter, for the quantity of bile derived from that source depends entirely upon the amount of effete matter separated from the living organism by destructive metamorphosis.

This is a very important consideration in the practice of medicine, for the physician ought not to expect to see much bile passing from the bowels when the patient is eating very little, and when the daily loss of weight by the body is but a few ounces. It is a very common error among the people to suppose that the bile is a morbid or unhealthy excretion. It is in fact one of the most healthy and regular of all the visible products of organic life. This is evidenced by the color, consistence, and odor of natural, healthy feces, so well

known by the commonest observers among all races of men. To understand fully the importance of a careful examination of the stools at the bed-side, we must first acquaint ourselves correctly as to what constitutes healthy feces.

The views which I am here presenting as to the functions of the liver, and as to the bile being formed out of effete matter, and bile being an excrementitious substance, were particularly inculcated in the medical department of Transylvania University. Benjamin Winslow Dudley, M. D., Charles Caldwell, M. D., John Estlin Cooke, M. D., and James Conquest Cross, M. D., all agreed as to the truth of the principles which I am here advocating. Dr. Dudley's lectures made a profound and lasting impression upon the large classes which heard him for years; but he has left very little in book-form.

Drs. Caldwell and Cooke were voluminous writers. I have quoted largely from Dr. Cooke's book. My friend Dr. Elisha Bartlett said to me that he considered Caldwell's Notes on Cullen's First Lines among the most philosophical writings he had ever read. Dr. Cross, as a writer, confined himself to essays. It is to these that I am about to refer. Dr. Cross may be said to have been a disciple of Dudley, Caldwell, and Cooke, and he expresses in these essays what he may be said to have adopted from each of them. The essays referred to were published in the Western and Southern Medical Recorder, 1842, of which Dr. Cross was the editor, and of which I was for a time assistant editor.

It is because I believe this whole subject to be so little understood that I make these references. It is inexplicable to me how the learning of the past has come of late years to be considered as nonsense.

The first medical writers in this country and in Europe may be said to ignore entirely the uses of the bile, which they admit when they declare that it is reabsorbed.

We copy from Dr. Cross's article entitled "Thoughts on the Biliary Secretion": "In regard to the vessels which enter and those which come out of the liver, there is a striking peculiarity

that should be particularly noticed, which is that the former are larger than the latter, and circulate more blood. This is not the case in regard to the other organs; those that return the blood from them are always larger than those that carry it to them. This is an important anatomical peculiarity, and it will assist in understanding the facility with which, under certain circumstances, the liver becomes diseased."

He states the following propositions, to which we shall allude as far as it will be necessary:

First. The blood of the arteries is different from that of the veins.

Second. The blood of the vena portarum and its tributaries is different from that found in the other veins.

Third. The elements of the bile are found chiefly, if not exclusively, in the portal blood.

Fourth. Portal blood is different from that found in the hepatic veins.

Fifth. Bile is less vital than recrementitial secretions.

Sixth. Bile is not concerned in the process of chylicification.

Seventh. The peristaltic action of the bowels is in no way dependent on bile.

Eighth. Bile is an excretion of the liver, consequently a depuratory organ.

Many of the questions discussed on these eight propositions I have already considered in former chapters. Dr. Cross assumes, as an undisputed fact, that the bile is formed from effete matter. I agree with him and with all other physiologists in this, that effete matter is separated from the living organisms by destructive metamorphosis, while I affirm that destructive metamorphosis only takes place when the body is losing in weight.

Dr. Cross says on the third proposition, "If bile is formed of effete matter; if this imparts peculiarities to portal blood, by which we are enabled to distinguish it from other blood; if the liver is a depuratory organ, and causes portal blood to part with some of its distinctive attributes during its transit through that organ, it is altogether probable that in passing through it, portal

blood experiences such a change as to enable us to distinguish it from that which is found in the vena cava, and which consists in great part of blood that has circulated through the liver. If, in other words, the liver, like the lungs, purifies the blood from injurious matters with which it is incorporated, we should see, as in the blood which returns from the latter to the heart, some evidence of it in the blood that returns from the former to the vena cava. That such is the fact, the proposition which we have just been considering seems very clearly to indicate. We have no reason to believe that caval differs from venous blood in general, and as it has been shown that there is obviously a difference between the latter and portal blood, it follows as a necessary consequence that portal blood and caval blood are not perfectly identical. One of the principal points in which portal blood and that of the other veins differs, is in respect to color; the former is said by many observers to be of much darker hue, and to contain, according to the doctrine for which we contend, a larger amount of effete or refuse matter. If this contains the elements of bile, and the dark color of portal blood is owing to its presence in it, it is evident, as the bile is separated from the blood in its passage through the liver, the blood must become of a less dark hue."

"That the blood of the vena portæ in circulating through the liver is exposed to a process of depuration, something like that exerted upon the blood in its passage through the lungs, can not be considered improbable. And, moreover, when we recollect the disparity that subsists between the size of the vessels that enter and those that come out of the liver, it must be very sensibly strengthened. The former are larger than the latter, doubtless because more blood enters that organ by the vena portæ and hepatic artery than returns to the general circulation through the hepatic veins, and consequently there must be some other outlet; and we know of no other than that furnished by the biliary ducts through which the effete matter or bile passes into the bowels. If the office of the liver, in common with that of the lungs, is to purify the blood of its refuse mat-

ter, it may be asked why does not the blood, in passing through the liver, experience a more remarkable change—why is it not as obvious as that which takes place in the lungs? To this question we would respond, that the wonder is not that the change experienced is not greater, but that it is so great. The blood of the vena portæ, after having furnished the elements of bile, and that of the hepatic artery, after having yielded nourishment to the liver, are received into the capillary ramifications of the hepatic veins, into which is also emptied the effete matter of the liver, which circumstance is, of itself, nearly sufficient to counteract any depurating action which may be exerted upon it by the liver. Besides, the liver and the lungs are differently situated. In the former the blood is merely decarbonized, but in the latter it is decarbonized and receives oxygen from the air; in the former the blood that has been decarbonized is immediately mixed with the refuse matter of the liver, but in the latter the decarbonized blood is not again mixed with the refuse matter until it reaches the general system, for the effete matter of the lungs passes into the capillaries interposed between the bronchial arteries and veins, and then the blood is allowed to return to the left side of the heart, purified in a great degree from refuse matter by the joint agency of the liver and lungs.”

On proposition fourth he says, “There are but two organs in the animal economy to which two kinds of blood are sent. They are the lungs and the liver; and in regard to the former, it is evident that one kind would not be sufficient to enable them to perform the functions that are dependent upon them. Blood is sent to them for the purpose of being arterialized, and blood of a different character to nourish them. One kind to be freed from its impurities, and another kind already in a state which fits it to maintain the physical structure of the organs.”

Now the object of this arrangement of the vascular systems of the lungs is perfectly intelligible, and yet it is not in any respect different from the vascular systems of the liver.

Two kinds of blood are sent to the liver for precisely the same purpose that they are sent to the lungs.

That through the vena portæ to furnish bile, and thus free the blood from effete matter; and that through the hepatic artery to nourish that organ.

The nourishment of the liver can not be ascribed to portal blood, for that would be wholly inconsistent with a fundamental principle of nutrition, which is that the nourishing fluid must be always furnished by arterial blood. The small size of the hepatic artery is of itself sufficient to refute the idea that its blood yields nourishment to the liver and also the elements of biliary secretion.

On the fifth proposition Dr. Cross says, “. . . And then, bile in the gall-bladder is certainly a very different fluid from the secretion that is found in the biliary ducts. In that reservoir it is found to possess properties very unlike those by which it is characterized just after it is separated from the blood. In the biliary receptacle, when it has been confined there for a considerable length of time, it is observed to be a green and sometimes of a black color—to have a very bitter taste, and offensive smell, a viscid consistence, frequently concreting, sometimes being converted into calculi, and apparently of a very heterogeneous composition; while that which is found in the biliary ducts is a thin, yellow, sweetish, inodorous, and, in some degree, a homogeneous fluid.” He says, “In inanition bile is rapidly and in great abundance formed, and it becomes under such circumstances very bitter.”

Bile formed under such circumstances must of course be formed entirely from effete matter.

“A sudden augmentation of bile, says Dumas, is an invariable consequence of those emaciations in which the fat rapidly disappears, and this is so uniformly and obviously the case that Thornell says that the liver eats the fat.”

“By Lorry, Thornell, Fourcroy, Dumas, and many others, a great similitude has been remarked between fat and bile. From those observers we learn, what experience has taught every attentive physician, that they exist in an inverse ratio; that when the deposition of fat becomes great and obesity is the result the

biliary secretion diminishes; and that when the fat is being removed by intestinal absorption, and emaciation produced, the afflux of bile into the bowels is rapid and abundant. They also remarked that the bile that flows during the decline of excessive *embonpoint* is of a degenerative character. It is the result of a very frequent, diversified, and abundant experience, that those cases of fever that do not emaciate, are very obstinate and difficult of cure. Now my attention has been particularly directed to such cases, and I assert from actual observation that the liver is always in a torpid state; that no improvement ever takes place until the liver is provoked to act, and then emaciation follows." . . . "Now when we have in mind the multitudinous and discrepant sources from which the vena portæ receives its supplies, we can not be surprised that its contents should vary considerably in character, or that portal blood should yield a secretion very complex and heterogeneous in its constitution and nature."

On the sixth proposition, Dr. Cross says, "If bile is formed of effete matter, which has been removed from the textures because of its unfitness to sustain their physical structure, it is absurd to allege that it is necessary to the proper assimilation of the nourishing fluid that has just been extracted from the contents of the stomach."

As to the bile mixing with the food after it passes into the duodenum, Dr. Cross says, "By Hunter it is said it does not appear that there takes place an admixture between the bile and the digested aliment. The former passes over the latter, never incorporates with it, and is expelled from the bowels in conjunction with the excrementitious part." This is the opinion of Abernethy. He remarks that bile does not incorporate with the digested aliment, "because the least quantity of bile will give taste and tinge to a very large quantity of other matter."

It had never occurred to Dr. Cross that the clear, sweet, inodorous bile was formed from the food, while the dark, viscid, offensive bile was formed from effete matter. I think I have clearly and conclusively shown the different sources of these two

kinds of bile. Modern physiologists agree neither with Dr. Cross nor myself.

“Dr. Cross says, “The particular terminus of the choledochus duct in most animals has been urged to prove that the bile is necessary to convert chyme into chyle. It should, however, be recollected that in some animals bile is not poured into the duodenum. In some of the molluscous animals the small bowels receive but a very small portion of the biliary secretion, and that too not at their upper but at their lower portion. The greater part of the bile is emptied either into the cecum, or very near the anus, at points very remote from the seat of chylicification. In such animals bile is very evidently an excretion.”

Dr. Cross concludes with the following remarks: “Without indulging in numerous details, we shall merely draw attention to a fact which is admitted by all, which is that diseases of the lungs are most common in high northern climates, while those of the liver prevail almost exclusively in low southern latitudes. The pulmonary and hepatic apparatuses are the two leading emunctories of the system, and are placed at the extremities of the venous circulation, where it is presumable such matters as should be excreted are accumulated in the greatest abundance. Now the excretory function of the skin is closely associated with the functions of both of those internal excretory organs, so much so indeed that a change in one for the better or the worse can not take place without being felt to a greater or less extent by the others. Thus if cuticular action is energetic, there will be an abundant hepatic secretion, but a sparing pulmonary exhalation; if cuticular action is enfeebled, there will be a languid secretion of bile, and a determination of fluids to the lungs; if there is diminished action of the hepatic, there will be increased action of the respiratory apparatus, and *vice versa*.

“External temperature is the most powerful known cause of these various modifications. When it is high, the skin and liver act freely, and then a diminished amount of excretory duty devolves on the lungs; but when it is much reduced, the functions of the former are languid, while that of the latter is energetic.

Overexertion of any organ is a common cause of subsequent enfeebled and often suspended action, and consequently of disease. Thus in cold climates, from the moderate functional exertion of the skin and liver, the principal excretory duty falls on the lungs, and hence the frequency of pulmonary complaints in northern regions; but in tropical climates this labor falls on the liver, and hence the frequent and fatal abdominal diseases so characteristic of southern latitudes. In temperate climates, such, for example, as that of Kentucky and most of the Middle States, the blood is freed from its impurities in about an equal degree by the liver and lungs, and consequently we have rarely either the pure liver- or lung-diseases so common in northern or southern latitudes.

“Both the hepatic and pulmonary elements are distinctly observable in a great majority of the cases of disease that occur, and the physician who does not keep these facts continually in mind, or is too exclusive in his pathology or therapeutics, will surely find to his cost that he has misapprehended the character of the enemy with which he has to contend.”

It has been ascertained that, as a general rule, those animals in which there is an extensive respiratory apparatus have a small biliary organ, and *vice versa*.

Then by Dr. Carpenter we are told that “Among the insect tribes the liver is but little developed, and it would seem that it is altogether subordinate to the urinary system, which has been erroneously considered a part of it. It is easy to account for the low condition of the liver in these classes; since the extensive amount of the respiratory function must render almost unnecessary any other method of discharging carbon from the system. It is not among all the articulata, however, that the liver appears a subordinate organ; for in the crustacea, whose respiration is aquatic, and therefore less energetic, it attains a very considerable size, occupying frequently a very large part of the abdomen, and in one or two species acquiring a spongy texture by the union of its minute subdivisions into one mass, so as to resemble in some degree the solid form which this gland presents in higher

animals. The liver usually presents in the mollusca a very large size and highly-developed condition, which may be fairly regarded as compensating for their feeble respiratory powers. The liver is of a reduced size in birds, but this apparent deficiency is not felt by them, as the respiratory apparatus presents an uncommonly extensive development. Between the liver and the lungs of the mammalia we do not observe that remarkable disproportion which seems to be characteristic of most of the inferior orders of animals; they are of medium size and in proportion to the bulk of the animal in which they are found. The depuration of the blood therefore, under ordinary circumstances, is devolved neither mainly nor exclusively upon the one or the other—it is the result of their joint agency, neither of them ever performing extraordinary duty, except from the force of known causes, and then disease is very apt to follow.”

Dr. Cross's remarks in the essays from which I have quoted apply principally to the formation of bile in the bodies of the sick. He taught, as I do, that effete matter is then separated from the solid organism; and we both agree that this effete matter is separated from the blood by the liver and lungs.

Dr. Cross has referred to the fact that in some of the severest and most fatal forms of fever there is no emaciation or loss of weight, and that no improvement in such cases is manifest until emaciation or loss of weight begins, which is always attended by a free pouring out of bile.

Dr. Benjamin Rush speaks of a similar form of fever in which there was no disturbance apparent in the regularity of the heart's action and when the temperature of the body remained natural. In such cases destructive metamorphosis is going on, and effete matter is separated from the living organism; but if it is not thrown out of the body by some gland, there will of course be very little loss of weight; only so much as is carried off by perspiration and by respiration.

Here again we find another proof that the kidneys do not remove the effete matter from the blood. Every physiologist knows, and no one of them will dispute the fact, that all glandu-

lar secretion is suspended during the exacerbation of fevers. It is a fact also equally well known to them, that during the continuance of a fever, there is a free or increased flow of arterial blood during that stage of fever, and during that only, which is called the stage of excitement or reaction, in contradistinction to the access, the chilly or cold stage, and the sweating stage. Now during all these stages, except the one of excitement or reaction, there is weakened action of the heart. This weakened action necessarily involves and produces congestion, more particularly of the abdominal viscera. Weakened action of the heart, with its concomitant congestions, necessarily involves a diminished flow of arterial blood; so that it would be only during the stage of febrile reaction that any large or even regular quantity of arterial blood would be sent to the kidneys. But it is during this stage, as I have said above, that no secretion or excretion takes place in the glands. No effete matter then could be removed from the blood by the kidneys during the stage of reaction, when there is an increased flow of arterial blood through them. Now bear in mind that in fevers where there is emaciation there must be a large amount of effete matter separated from the organism.

During which stage of the fever, I then ask, do the kidneys remove it from the blood? I have shown that during one of the stages they can not remove it, because no glands either secrete or excrete during the stage of febrile excitement. Perspiration even is suspended at the time. I have also shown that during the other three stages there is a diminished flow of arterial blood to the kidneys as well as to the other organs. The liver, as a gland, is subject to the same law. But there is this remarkable difference between the liver and the kidneys: during all the stages of fever in which there is weakened action of the heart, with the attendant and unavoidable congestions, there is an increased volume of blood within the cavities, and particularly in the veins which carry blood to the liver. The more completely the action of the heart is weakened and the greater the congestion in the abdominal veins, the larger is the amount of blood which is pressing upon the liver for transit through it.

Dr. Cross says—which I know to be the fact—that apparent improvement begins in the cases to which he referred so soon as there is a free flow of bile. This I maintain, as he does, is because the liver separates the effete matter from the blood and pours it out as bile into the bowels.

This is the formation of bile out of effete matter. Healthy reaction, not febrile excitement follows this exit of bile from the body. Now during all this time very little urine is separated from the blood by the kidneys and poured out into the bladder; not enough certainly to account for the loss of effete matter, when there is great emaciation.

Dr. Cross says there is no improvement in fever cases until the patient begins to emaciate or lose in weight, and that during that condition the action of the liver is suspended. Such a state of things may, and often does, exist. Thousands and tens of thousands of individuals die annually because the liver is not made to separate the effete matter from the blood—which is a poison in and of itself—and to pour it out into the bowels in the form of bile. That the liver can be made to secrete bile and to pour it out I will show in another chapter. I now refer to the action of cathartics, and particularly of calomel. I have shown in the preceding chapter that it can be done by emetics.

It can not be too constantly borne in mind that destructive metamorphosis never goes on in the human body, and that consequently effete matter is never separated from the living organism, except when there is a disturbance in the functions of organic life. It is always a pathological condition, and its removal from the body involves an increased labor of some organ. As I have before said, the natural and healthy function of the liver, in a state of perfect health, is to remove certain constituents of the food which ought to be separated from it, and to pour them out as bile into the bowels. Whenever, then, the liver has to create bile out of effete matter, it is extra work thrown upon it. The existence at all of effete matter in the blood is a thing pernicious in itself. Ordinarily it is known by the commonest observers of hygienic rules. The liver

will go on to do this work without the individual knowing that he has a liver; but if, to the difficulty of the existence of effete matter in the blood, there is added a fever poison, such as malaria, then the trouble is increased. The healthful action of the liver, as of all other glands, depends upon a due and regular supply of pure arterial blood. Any fever poison, or proximate cause of disease, produces a disturbance in the entire system of organic life. The liver feels this disturbance as well as any other organ, and as it has extra work to do when it is thus incapacitated, it often has to be stimulated or forced to perform the labor thrown upon it. When the limbs complained of the belly, in Æsop's fable, they did not know that if one of the organs in the belly (the liver) did not work they would all soon be dead.

If bile, then, is formed from effete matter, and if effete matter is never separated from the living organism, except when the individual is indisposed or sick, it must be apparent to every one who will reflect, that there is no gland in the body, upon the regular performance of whose functions good health so much depends.

I will request the reader again to examine and to consider what I have copied from Dr. Cooke on congestion. The multitudinous complications in all forms of sickness resulting from congestion within the cavities, and particularly of the abdominal viscera, can only be relieved by the removal of effete matter from the blood. It is the especial duty of the liver, under such circumstances, to do this work. Cathartic medicines accomplish very little unless they contribute to this end.

The removal of the feces from the bowels, considered simply as the removal of what remains of the food, is perhaps the smallest part of the service performed by them when judiciously prescribed. But there are many other pathological conditions in which there is no very apparent disturbance of the regularity of the heart's action, and in which there is no obvious congestion in the veins of the abdominal viscera, yet because the liver fails to remove the effete matter as fast as it is separated from the living

organism, there occur irregularities in other organs, so that they do not perform their proper functions. This often amounts to what may be called a disease.

I will give an instance. It often happens that there is some trouble in the urino-genital system. The remote cause may be a lesion, as, for instance, stricture in the urethra, or enlargement of the prostate glands. Now some persons will go for many years without any particular trouble on account of their lesion, but a time comes when the lesions cause an obstruction in the passage of water from the bladder. Sometimes it is impossible to remove this water by the catheter. It will be found that medicines which are supposed to act specifically upon the bladder or any part of the urinary system only aggravate the evil when they are administered. In such cases if the liver is roused to action and made to secrete or pour out bile freely the whole difficulty will be promptly and often entirely removed. This is altogether in accordance with the doctrines which I am so earnestly endeavoring to teach. There is one condition in which it may be said that constructive and destructive metamorphoses do go on at the same time when the animal is in good health. I allude to the training of a race-horse or an athlete. The exercise and other training cause the animal to throw off fat, and what is commonly called cellular substance. Of course these are removed from the body as effete matter. On the other hand, the muscles are more fully developed, and the animal becomes stronger and more active. I do not think this training can be called a state of perfect health.

In my assertion that effete matter is not separated from the living organism when the animal is in perfect health, I am considering both constructive and destructive metamorphosis simply as organic or vegetative functions. Neither the horse nor the man would lose in weight—that is, have the effete matter thrown off—unless the organic or vegetative functions were brought under and subjected to the control of the organs of animal life. But it can not be made to appear that the muscles increase in size and grow stronger until after the fat and cellular substance have

been removed. We know that there is one condition in which only one of these processes can go on. When either a well or sick man is losing weight daily without eating any thing, constructive metamorphosis is certainly not acting. No physiologist can deny this.

Now if it can be shown, as this case certainly shows, that destructive metamorphosis can go on, and does go on alone, and is a single force, is it not incumbent upon physiologists to show that such is not the fact? Look at the vegetable world. Is there a single instance in which construction and destruction go on in the same body at the same time? Consider the world of plants. Take the seed of the cucumber. It sprouts. It forms a vine and blossoms. After a few days we have a full-grown cucumber. The whole plant, including the fruit, has fashioned the sustenance or food which it drew from the inorganic world into a luxuriant vine and cucumbers. During its growth the plant throws off carbonic acid, but that was not a part of its living organized tissue. It was one continual growth, one continual exercise of the force of constructive assimilation. But after a while things change. The leaves begin to wither, the cucumbers turn yellow, and in a few weeks or months all rot and return again to the inorganic world. Thus destructive metamorphosis, and it alone, is operating upon the plant. If this law exists in plants, it exists also in animals. No physiologist will deny this.

It is because organic life in man is so much under the control of the functions of animal and intellectual or mental life, that we find it almost impossible to say when constructive assimilation ceases, and destructive metamorphosis, or the separation of effete matter, begins. They do not go on with the regularity of ebb and floodtide; but like the tides, the one force must cease to move before the other force begins.

In a former chapter I have clearly stated that when constructive assimilation is alone active, and there is no separation of effete matter from the tissues, the liver and other glands may and do act upon effete blood. But this applies more particularly to

grown individuals who do not increase in weight when the food passes through the system without adding any thing to the material organism.

Dr. Dudley adopted the practice of Mr. Abernethy in the treatment of diseases generally, and that of John Hunter in the treatment of certain maladies. He did not trouble himself much with physiological or pathological considerations. He agreed, however, with Dr. Caldwell, who was more speculative, that the secretions and excretions should always be considered and attended to. Dr. Cross considered Dr. Cooke to be the *Magnus Apollo* of the medical profession, though he did not follow his practice.

It was the enormous quantities of cathartic medicines given by Dr. Cooke which brought both his theory and practice into disrepute. When Dr. Cooke was examining the writer—in the greenroom—he told the learned professor that he did not agree with him as to his practice. The exclusive use of cathartics is not a necessary corollary from Dr. Cooke's theory of congestion and of the functions of the liver. I think the medical world would be benefited by a republication of his two books. Dr. Caldwell's great work was his essay on "The Temperaments."

CHAPTER XIII.

CALOMEL.

Calomel is classed among cathartic medicines. Before we inquire into its uses as a therapeutic agent, we must inform ourselves as to what we expect to accomplish by any medicine belonging to this class. I have before shown, in the chapter on Emetics, that the vomiting up of undigested food remaining in the stomach is among the least of the benefits to be expected from medicinal emesis; and I would now state that the removal of the *débris* of the food from the bowels is among the smallest benefits to be expected from a cathartic judiciously prescribed. For such a purpose calomel should never be used. There are some diseases, as dropsy, in which it may be desirable to excite the pouring out of the serum into the bowels; but there is no disease with which I am acquainted in which any therapeutical benefit is to be expected by the pouring out into the bowels of more than a natural quantity of mucus.

Many cathartics excite a greater flow of mucus than that which covers the internal coat of the bowels in health. But while this is sometimes inseparable from the action of cathartics, it is in no case desirable. If nothing more is to be expected from the action of cathartics than the removal of mucus and desquamated epithelial cells, then castor oil is the best of all cathartics, and I know of no reason why any other medicine of that class should ever be given. We might, instead, rely on the lavements of the French, or injections or clysters of the English.

The one or the other will accomplish all that can be effected by a cathartic, if the removal of the substances spoken of is all that is to be accomplished. Until recently it was supposed that cathartic medicines excite the liver to pour out bile freely into the bowels. Before the discovery of calomel, aloes and rhubarb were among the medicines used by the ancient physicians with this end in view.

The Greeks and the Romans speak of seeing black, green, and yellow bile in stools, and state that they followed the administration of certain medicines. Physicians, from their day to the present, have said the same thing without there being any mental reservation or conception that what they said was not true. But things have changed, and what a change!

A few years ago a committee of physicians was appointed in Edinburgh to determine the fact whether calomel or any other medicine excited or stimulated the liver to secrete and pour out bile. They made numerous experiments on dogs, and perhaps some other animals. The conclusion at which this committee arrived is expressed by Sydney Ringer, M. D., of London, as follows:

“First. That neither blue-pill, calomel, nor corrosive sublimate affect the liver unless they purge or impair the health, when the quantity of bile is diminished.”

“Second. Purgation from any cause lessens the amount of bile and the proportion of its constituents.”

It is said also by this committee that doses of podophyllin, varying from two to eight grains, diminished the solid constituents of the bile, whether they produced purgation or not, and that doses that produced purgation lessened both the fluid and solid constituents of the bile.

Dr. J. Hughes Bennett was the chairman of that committee. No report made by a committee of physicians ever made a more decided impression upon the medical world than has the one of which Dr. J. Hughes Bennett is the author.

Job said, “O, that mine enemy had written a book.” I do not consider Dr. Bennett to be my enemy, but I class

him among *hostes humani generis*, and therefore rejoice that he has written a book. I copy from his book entitled *Clinical Lectures on the Practice of Medicine*, to show what he supposes has been accomplished by the medical profession as to the therapeutical value of medicines.

“First, then, are there many drugs or medical preparations whose effects are unquestionably beneficial in particular diseases? I think there are, as witnessed by the influence of, first, quinine in ague; second, pitch ointment in psoriasis; third, male shield-fern in tapeworm; and fourth, sulphur ointment in scabies.

“I think no one who has used these remedies in the diseases named can doubt their curative power as a matter of fact, however they may differ as to the mode of their action. Of a similar unquestionable character are the following, although, for the reasons previously stated, the first two should be placed among the dietetica, and the third among the hygienica. They are, fifth, cod-liver oil in scrofulous and tubercular diseases; sixth, lemon-juice in scurvy; and seventh, constant moisture in eczematous and impetiginous diseases of the skin.

“These seven remedies I put into the first, and the following into the second class: eighth, colchicum in acute gout; ninth, iodide of potassium in certain forms of periostitis; tenth, iron in chlorosis and amenorrhea; eleventh, arsenic in scaly skin diseases; twelfth, copaiba and cubebs in gonorrhoea; thirteenth, nitro-muriatic acid in oxaluria; fourteenth, super-tartrate of potassa in Bright’s disease, with diminished urine and dropsy; fifteenth, oils and fats in parasitic diseases of the skin.

“These remedies, though not so valuable, must, I think, be admitted to be also curative in certain cases by the majority of practitioners.

“As to blood-letting in inflammation, mercury in syphilis, and iodine in scrofula, I consider their value, though highly lauded by some, to be more than questionable. The fifteen remedies named, therefore, I consider to constitute all the positive agents we possess capable of curing disease or morbid states when they are once fairly established. Many other drugs re-

lieve symptoms—are palliative and most useful during our treatment of disorders—but I repeat, as curative, I shall only be too happy to hear of any I have omitted.” (Pages 335, 336.)

The cheerless and gloomy prospect by which the wandering Jew was surrounded when he stood on the frozen shores of Behring Straits, was not more desolate and forbidding than the picture of the fifteen remedies enumerated by Dr. J. Hughes Bennett as the total result of all that has been accomplished since the days of Hippocrates in a department of knowledge which assumes to be called medical science.

John Stuart Mill could hardly have had this picture before his mind’s eye when he maintained, in opposition to M. Auguste Comte, that medical science was among the highest, if not the highest and most difficult departments of human knowledge. Who can wonder at the popularity of homeopaths in the most intelligent communities when such a picture is presented to cultivated and thinking men by a London medical *savant*. The disciples of Hahnemann could say, we are thinking men even if our infinitesimal doses are but an offshoot from the metaphysics of St. Thomas Aquinas, who wrote a book to solve the problem as to how many angels could dance on the point of a cambric needle without jostling each other. This picture takes us back to the Greek empirics, who eschewed and rejected all explanations of the action of medicines, which they called theoretical absurdities. If such is the sum total of the practical part of the physician’s art, then all attempts to find out the laws of healthy life, all attempts to find out the departure from those laws, and the causes of them, all attempts to find out the therapeutical action of remedies, are but so much labor thrown away; and we had just as well follow the teachings of a Fetich or Indian doctor.

But let us consider the qualifications of Dr. J. Hughes Bennett for making the report which has made so profound an impression on the medical world. A man ought to know something about a thing before he can be supposed to be qualified to make a report upon it. What did Dr. Bennett know about the bile? He believed that bile is not formed out of effete matter.

He believed that cholesterin, one of the excrementitious principles found in the bile, is not formed in and by the liver. He believed that urea and other excrementitious principles found in the urine are not formed by or in the kidneys. He believed that the living organisms, such as nerve, muscle, etc., not only have the power to form and to re-form themselves, but that they also have the power inherent in them to form or create excrementitious proximate principles, such as cholesterin and urea; or that the capillaries have the power to perform both of these functions. He believed that urea and cholesterin are normal constituents of certain organisms, as of the muscles in the one case, and of the crystalline lenses and the nerves in the other. He believed that all of this was accomplished under the double working of constructive assimilation and destructive metamorphosis going on in animal bodies at the same time. He believed that the only office of the kidneys was to separate from the blood proximate principles which had been formed somewhere else. He believed that the kidneys in this way removed all effete matter from the blood, and that the only channel of exit for it was through the bladder and urethra. He believed that the bile was not a constituent part of the feces, and therefore that it was not an excrementitious fluid. He believed that the liver only separated cholesterin from the blood after it had been formed somewhere else. He believed that the bile was reabsorbed, and therefore that it was a recrementitious substance.

I say that he believed all this because it is so taught in the books, and he does not intimate that he rejects their authority. I say that believing all this, he did not know under what circumstances the bile ought to be expected to flow freely. He had never conceived the idea that the largest quantity of bile is formed from the food when the animal is in good health. Not knowing this, he could not see, or did not see, how the administration of calomel or any other drug would interfere with digestion and the absorption of food, and thus prevent a free flow of bile immediately after eating, and while digestion was going on. He had no conception of the fact that when the bile is not formed

from the food, the only other condition under which it is formed is when the blood is charged with effete matter, and that this effete matter must be removed from the blood by the liver as bile.

If the calomel or podophyllin was given to dogs in good health it would make them sick, and thus interfere with the proper digestion and absorption of food. This would prevent the normal and regular formation of bile from the food, or vitiate it and alter its character. If the drugs were given to sick dogs, they were not the medicines suited to the nature of dogs, for they eat grass when they are sick, and select particular kinds of grasses. So the experiment in that case would be worth nothing. Deer eat tobacco for food, and hogs, it is said, are not poisoned by arsenic. Experiments on the inferior animals prove nothing as to the therapeutical effects of medicines on human beings. Medicines, properly so called, make healthy people sick, because they interfere with and interrupt the digestion of food and the conversion of it into blood. They relieve the sick or are curative when they remove the impediments to healthy digestion. Such certainly is the method of cure, so far as emetics and cathartics are concerned. These medicines should only be given to regulate and to correct the secretions and the excretions; and to know when and how to give them is what constitutes the skillful and successful physician. Such considerations do not enter into the vocabulary of Dr. J. Hughes Bennett's art.

Not one of the fifteen medicines named by him exerts any direct influence upon either the secretions or excretions. A man believing as he does can not understand why calomel given in sufficient doses promptly arrests and cures cholera infantum and Asiatic cholera. If he were to stand by and witness such cases, he might be compelled to admit the fact that the patients got well after taking the medicine, but he would have no conception as to how it was accomplished. It would be to him simply an empirical fact. No report ever made out by a committee of physicians could throw greater discredit upon experiments made on the inferior animals, and by vivisections than ought this one to do. It should convince all men, and particularly physicians,

that experiments so made and of this class are almost always not only valueless for good, but that they are deceptive and injurious. The practice which is founded on this report and its concomitant teachings has caused the death of tens of thousands of children, who died from cholera infantum, and of an equal number of adults who died of Asiatic cholera, nine tenths of which ought to have been cured.

I am well aware of the mountain of difficulties against which I have to struggle. I know the intense prejudice of the people against calomel, and I know that physicians have contributed more than any other men to this prejudice. Scientific men will denounce me for trying to overturn their theories, and I will be held up to the world by them as an ignoramus; and all the little dogs will bark at my heels, and will yelp! yelp! "big doses of calomel." All sorts of lying quackery will present my book to the world as another production by what they call an allopathic physician in defense of the administration of poisons.

As to the use of calomel at the present time by scientific physicians, I copy from one of the most recent standard works. Edward John Warring, M.D., F.R.S., in his work on Practical Therapeutics, says, "Amongst the revolutions in modern medical opinion, few, if any, are more conspicuous than that which relates to calomel, not only as a cholagogue, which is noticed elsewhere, but as an antiphlogistic possessed of special powers in controlling and subduing inflammation, especially of serous membranes. For upwards of half a century after its claims in combination with opium were prominently set forth by Dr. Hamilton of Lynn Regis, in 1783, it maintained the highest repute, which is the more extraordinary from the circumstance that it was employed almost indiscriminately in almost all diseases of an inflammatory type. With the change which has come over our views as to the nature of inflammation, mentioned in considering tartar emetic, has ensued a change in practice, and calomel has fallen from its high estate, and there is now a danger of this remedy, possessed beyond a doubt of powerful virtues, falling into undeserved neglect." (Page 318.)

As to the cholagogue powers of calomel he says, "In diseases of the liver no remedy has been employed so generally and indiscriminately as mercury; and up to a very recent period its cholagogue virtues were firmly believed in by the majority of the profession. The belief in its powers in this respect has been upset by the experiments of Dr. Bennett and others (*ante*); and apart from this it has been superseded in the practice of many by podophyllin, which is thought to possess all the virtues without the disadvantages of mercury. Under these circumstances there is a great chance of mercury, in hepatic affections, falling, as other medicines have done, into undeserved neglect. There are cases, e. g. congestion of the liver, in which a full mercurial (calomel) purgative is of undoubted utility. Under its administration the local weight, pain, fullness, and other symptoms subside simultaneously with the occurrence of copious bilious stools, and it is impossible to disabuse the minds of those who have repeatedly witnessed those simultaneous effects of the idea that they bear intimate relation one to another."

"The most plausible, and probably the true, explanation is that given by Dr. Murchison (pages 126, 404); namely, that mercury in these cases acts by irritating the upper part of the small intestines, propelling onward the bile as fast as it is poured into the duodenum, thus preventing its reabsorption, and that the biliary accumulations thus excreted, constitute the 'bilious stools,' which have generally been regarded as the result of increased biliary secretion from the liver itself." But as Dr. Murchison observes, "If the calomel acted by stimulating the liver to increased secretion, it would be injurious in cases of hepatic congestion."

In addition to the above explanation, Dr. Murchison considers that mercury may perhaps also act by stimulating the gall-bladder and bile-ducts to contract through reflex action. By adopting Dr. Murchison's hypothesis, it is easy to understand how it is that jaundice sometimes yields rapidly to a calomel purgation (grs. v.) followed by a saline draught, and also how it will prove useless, and probably injurious, when the affection

is connected with obstruction by gall-stones or organic diseases of the liver. (American edition, pages 324, 325.)

It would be too great a departure from my subject to notice even the changes in medical opinion as to the nature of inflammation, which are supposed by Dr. Warring to have contributed in some measure to the disuse of calomel in the treatment of inflammatory diseases; but to a simple-minded man like myself, it seems difficult to understand how a change in the theory could alter the facts. If calomel for half a century, given almost indiscriminately, was found to be curative in the treatment of such affections, I can not see how it should be to-day less curative, even if it was formerly given under an erroneous theory. I deny the truth of the theory which rejects it. I have referred to the pious Scotch doctor, who said that with a phial of calomel and the blessing of God, he could cure all the children in the kingdom.

Dr. Warring accepts the theory that the bile is reabsorbed, and he is therefore bound to admit that it is a recrementitious and not an excrementitious substance, yet he allows that it does sometimes pass through the bowels. He also admits that in some forms of disease the patients are benefited by the passing off of the bile. This is altogether unintelligible to me. If the bile is a recrementitious principle, then it ought to remain in the system, as much so as the saliva, gastric and pancreatic juices. I did not know that the throwing away of saliva by spitting was ever thought to benefit the health of any body. I know it makes tobacco-chewers thin. If the theory of absorption is true, then any thing which prevents absorption and causes the bile to pass out of the bowels ought to be injurious, for it must disturb the natural and healthy functions. But Dr. Warring admits that the bile does pass off under many different conditions, and I do not think it is too much to say that he admits that patients are benefited thereby.

Dr. Warring seems to adopt as unquestionably true the supposed demonstrations of Dr. Bennett; but he undertakes to

explain the difficulty in which he finds himself by accepting a theory of Dr. Murchison. This learned and truly distinguished physician supposes that mercury in these cases acts by irritating the upper part of the small intestines, propelling onward the bile as fast as it flows into the duodenum, thus preventing its reabsorption. It seems to me that if this hypothesis were true, small doses of castor oil repeatedly given would as certainly push the bile out of the bowels without disturbing the equanimity of the system by the irritation of the duodenum. But Dr. Murchison himself seems not to have been satisfied with this first explanation of the action of mercury or calomel, for he gives us another. He supposes the mercury may, perhaps, also act by stimulating the gall-bladder and bile-ducts to contract through reflex action.

For a long time, when a physician was unable to explain some difficulty in a case, he concealed his ignorance by saying that the thing was caused or produced through sympathy.

To-day the same ignorance is covered up by the words, reflex action. It is used continually by men who assume to be scientific, who could not, if their life depended upon it, tell what they meant by it.

Notwithstanding the high authority of Dr. Murchison's name, the explanation here given is an absurdity. If calomel excites or stimulates the liver to secrete and to pour out bile, as I know it does, why suppose that the therapeutic effect is the result of reflex instead of direct action?

But Dr. Murchison also supposes that if calomel acted by stimulating the liver to increased secretion, it would be injurious in cases of hepatic congestion.

Whenever the liver itself is diseased, calomel, if given at all, must be given very cautiously. In those forms of disease in which calomel acts most beneficially and curatively, as in cholera infantum, Asiatic cholera, and miasmatic fevers, and many forms of inflammation, the liver is itself not diseased; but is only functionally at fault in the secreting and pouring out of bile.

We give calomel in all such cases to stimulate the liver to increased work, and we continue to give it until the difficulty is

removed. But, if bile is a recrementitious principle, if the physiological function of it is that it should be reabsorbed; if it is not formed from effete matter, then calomel should never be given under any circumstances. Not only so; if I am entirely wrong, then an emetic should never be given except to throw off undigested food from the stomach to relieve a colic; and cathartics should never be given except to remove accumulated feces from the bowels. I repeat, if I am wrong, and the scientific medical world in opposition to me is correct, then, with the two exceptions which I have named, the homeopathsists are right, because the infinitesimal doses which they give do not disturb the equanimity of the system.

Dr. Warring admits "that cathartics are used to excite increased biliary secretions, and to stimulate the action of the absorbents in all parts of the body. To promote the discharge of other secretions; thus, the previous use of purgatives often apparently promotes the action of diuretics." He says, "Most cathartics operate on a particular part of the intestinal canal; thus calomel, jalap, and colchicum stimulate the duodenum, and promote the discharge of bile."

Dr. Warring says, "Purging, observes Sir T. Watson, is an expedient which, in cases of violent inflammation or high general fever, should scarcely ever be omitted. To keep the bowels what is called open, forms indeed a part of the antiphlogistic regimen; but in acute inflammatory diseases, active purging is of very great service." (Pages 660, 661.)

It is difficult to understand the last quotation, when we know that Dr. Warring accepts the experiments of Dr. Bennett as conclusive; and also that he accepts the theory of the absorption of the bile. But yet he and many other distinguished medical writers who agree with him, speak of the bile passing from the bowels in different diseases, and admit that the patients are benefited thereby.

Now I think that no physician can dispute that if calomel does not excite the liver to action no other medicine does. The medical gentlemen to whom I refer adopt a theory that the kid-

neys remove the effete matter from the blood. I say that, if they are correct, cathartics should never be given; and this applies particularly to calomel, which sometimes produces injurious effects. It is because cathartics, especially calomel, stimulate the liver to the increased secretion and pouring out of bile; it is because this bile is formed out of effete matter; it is because the removal of effete matter from the blood, by the liver, lessens the congestion of the abdominal viscera; it is because the capillary blood-vessels (the real absorbents) are then enabled to take up more rapidly effete matter, as it is separated from the living organism by destructive metamorphosis. I say that it is because of all this that the many afflictions humanity is heir to (and particularly those referred to by Dr. T. Watson), which are benefited by cathartics, are relieved and cured.

Dr. Warring says that "the previous use of purgatives often apparently promotes the action of diuretics." This is not only so, but it often happens that a free discharge of bile, following a dose of calomel, enables the kidneys to secrete urine plentifully from the blood without the administration of any remedy belonging to the class of diuretics. Now this is altogether unintelligible, according to the theory that the kidneys remove effete matter from the blood; but it is as plain as a nose on a man's face, according to the theory of Dr. Cooke and according to my theory inclusive.

The renal veins, which empty into the vena cava, have no valves, and when there is congestion in the veins of the abdominal viscera, this congestion extends through the renal veins into the kidneys.

The congestion of the abdominal viscera, for the most part, is a coexisting condition when destructive metamorphosis is going on, and when a large amount of effete matter passes into the venous circulation. The removal of this effete matter by the liver relieves the common congestion of the venous cavity, and the particular congestion of the renal veins. The kidneys being thus relieved, re-commence their healthy action in a vigorous manner. But if they alone remove effete matter from the body,

then if a man, in every form of disease in which there is congestion of the abdominal viscera, should urinate by the gallon, it would not relieve the congestion. This is another proof of the absurdity of the belief or theory that the kidneys remove the effete matter. We know, where the congestion spoken of exists, that the kidneys secrete urine very sparingly.

To this the disease called diabetes is the only exception. I believe that all regular physicians admit that congestions of the brain and apoplectic seizures are most certainly relieved by cathartics; and I do not doubt that many of them will acknowledge that this relief is the speediest and most apparent when bile is copiously discharged with the stools.

Now I assert that calomel is the most effectual and certain remedy in the affections of the brain referred to.

If the reader will refer again to the extracts from Dr. Cooke, he will see that congestion of the brain can not exist without coexisting congestion of the vena cava and of the abdominal veins. (I am not now referring to arterial hyperemia of the brain.)

Calomel in these cases causes the liver to separate the effete matter from the blood. By this not only is the congestion of the brain in particular removed, but also the general congestion, and every organ in the body is enabled to perform its functions more healthfully.

In the spring of 1861 I wrote an article on the facts, published in the newspapers, as to the cause or causes of the death of Count Cavour, and sent it to Rev. W. W. Hall, M.D., for publication in his *Hall's Journal of Health*.

It was supposed that Count Cavour died from congestion of the brain. I thought it more than probable that the immediate cause of his death was imprudent if not excessive blood-letting. In that paper I discussed the question of congestion of the brain, and also when and why a man should be bled. In the latter I agreed with Marshall Hall. Rev. W. W. Hall, M.D., selected from my article such parts as he liked, and published them as his own, without so much as intimating that he ever heard of such a man as myself.

Afterward, when I complained of this treatment, he said that he did not publish communications in his journal, but only what he himself wrote. But it seemed that his mind was not easy about the matter, so he published the next year a short extract from my article which he had before omitted, in which I said that in a case such as I supposed Count Cavour's to be, I would have given sixty grains of calomel. He was willing to publish my name in connection with this avowal.

Well, I am ready to stand by that to-day. I am satisfied that I and the school to which I belong not only know when to give calomel, not only know for what reason we do give it, but also know how much to give.

I am perfectly satisfied that the reason why physicians generally entertain so many erroneous opinions as to the therapeutic virtues of calomel, is because they give it in quantities too small to make any appreciable impression on the liver; and because they do not know that small doses of calomel excite too much the mucous membrane of the stomach and bowels, while scruple and dram doses not only quiet the bowels but also allay the irritability of the whole system.

I may give a few cases which have come under my own observation, but, as I am not writing for fame, but with the hope that I may prevent the untimely death of thousands, I refer again to Dr. Cross.

In the *Western and Southern Medical Recorder*, for 1842, in an article, *Notes on the Sedative Action of Calomel*, he says, "We have for more than twelve years practiced upon the principle that the effects of a large dose of calomel are different from those that result from a small one. By a large dose we mean twenty, and by a small one five, grains.

"Although we urge no right to the original suggestion here alluded to, as we frequently meet with practitioners who are ignorant of the fact, or unwilling to acknowledge its importance, we shall not be trespassing upon the indulgence of our readers by pressing its claims with such facts as we have observed, and such arguments as may occur to us.

“We rarely ever use calomel as a purgative. It is slow and uncertain in its action, and therefore much more inefficient than many other articles to be found in the class of cathartics. When the secretions of the chylopoetic organs are deranged we always give it for their rectification, either alone or with some purgative, for the purpose of increasing its activity. With disorders of the organs referred to, there is commonly associated an irritable condition of the alimentary tubes, and when this is the case, we have uniformly found that a scruple or half a dram of calomel is much more effectual in moderating inordinate activity of the intestinal tube—in arresting the copious serous secretions of the bowels, almost always present under such circumstances, and in provoking the liver to action, which is generally at fault—than any smaller quantity.”

Twenty grains will commonly bring down two or three consistent and bilious evacuations, while five or ten will usually increase the peristaltic action of the bowels, and will cause the evacuations, by irritating the stomach and bowels, and thus augmenting the serous and mucous secretions, to be frequent, liquid, and exhausting. It is in cases of irritable bowels, in which frequent watery stools are passed, that calomel, either alone or in combination, is always given, and it is therefore of the utmost importance to know what quantity is calculated to do the most good. On this point my mind has long been made up, and I am satisfied that my success in treating intestinal complaints has been mainly owing to the fact that I have kept constantly in view the marked difference in the effects that result from large and small doses of calomel. Indeed, so thoroughly am I convinced that the efficacy of this mineral mainly depends on this circumstance, that I am not surprised at the very contradictory effects it has produced in the hands of those who rely upon five- or ten-grain doses. That they should have little confidence in it, is not wonderful, for they give it, as we believe, in quantities that are calculated to do more harm than good. . . . Nor does the mode of the action of the calomel, which we desire to illustrate, rest upon our authority. It has been re-

peatedly observed and verified in actual practice by those who have enjoyed the most favorable opportunities for arriving at truthful results.

Mr. Abernethy, who practiced many years in India, says, "Small doses of calomel, from two, three, four, etc. grains, will purge and keep up a considerable degree of irritation in the stomach and bowels, when twenty grains will not, but on the contrary will allay the irritation of both."

On the same subject Dr. Johnson says, "I shall prove in the course of this essay, what, indeed, is well known to many of our brother officers who served in India, that twenty grains of calomel will act as a sedative, and so far from griping and producing hypercatharsis, it will soothe uneasiness, and rather constipate than purge." Again he says, "I have myself taken calomel in twenty-grain doses three times a day, without experiencing the slightest inconvenience from the quantity; nay, I often found large doses set easier on the stomach and occasion less irritation in the bowels than small ones."

By Dr. Merrill, of Natchez, Mississippi, it is said, "Calomel, when given alone, I have always found to produce the best effects when given in scruple doses. A smaller quantity than this operates more frequently, producing more irritation of the stomach and bowels, and causing frequent watery dejections, which rapidly debilitate the patient."

Dr. Cartwright, of Natchez, and afterward of New Orleans, remarks that "Those who have not used calomel extensively would be apt to suppose *a priori* that large doses would produce hypercatharsis and debilitate the patient; but experience can best refute such suppositions, for it shows us that large doses of calomel operate much more mildly than smaller ones."

The late Dr. Armstrong says, "Small doses of calomel, from two to five and six grains, will purge and keep up a considerable degree of irritation in the stomach and bowels, when twenty grains will not; but on the contrary, will allay the irritation of both when it results from inflammation of their mucous surfaces.

Dr. Thomson, in his work on *Materia Medica and Therapeu-*

tics, informs us that "it often happens that small doses of calomel can not be retained in the stomach when this viscus is in an irritable state, although it retains large doses which act as a sedative." My own experience has afforded me numerous illustrations of this remark.

Dr. Lewis, in a very recent article on Tropical Dysentery, speaks in terms of great praise of the treatment followed by Dr. Heyn at Java in that disease, which consisted in administering at the very outset of it a scruple of the chloride of mercury. According to his experience, this powerful pharmaceutical agent, when employed in small doses, acts as an irritant, but when given in large quantities, has so decided and immediate effect as to merit the name of a direct sedative.

The concurrent testimony of so many respectable physicians, who have deduced their opinions from actual observation, can not fail to satisfy all that the effects of a large dose of calomel are very different from those of a small one, and that while the former allays the irritability of the bowels, the latter rather augments it than otherwise.

Dr. Cross gives a number of cases which came under his own observation confirmatory of what is written above. I have only space to give one of them:

"In 1829 a lady in the first months of pregnancy was afflicted with excessive vomiting, so much so indeed that her friends and physician became apprehensive that she could not live. After other expedients had been used, small doses of calomel among the rest, she took a dram of calomel at a single dose, and the irritability of the stomach was at once calmed, and it did not return again to such an extent as to cause uneasiness."

There are hundreds of physicians now practicing in the Mississippi Valley who would confirm the truthfulness of the positions taken by Dr. Cross; but the number of them is growing smaller every year. The younger physicians can but receive as the highest scientific truth what is taught in the first medical schools in this country and in Europe. Being taught that the kidneys remove the effete matter from the blood; that the bile is

a recrementitious principle; and that its healthy physiological function is to be reabsorbed; and receiving as the Q. E. D. of geometry the demonstrations of Dr. J. Hughes Bennett, it is not singular that they can not understand why calomel should ever be given; and when they do prescribe it, as directed in the books, they give it in very small doses often repeated; finding that their patients are frequently salivated, it is not strange that they should prefer podophyllin or any thing else in preference to mercury.

And here I would remark, in passing, that no physician knows any thing about the therapeutical or curative effects of calomel who ever intentionally salivated a patient, except, perhaps, in the treatment of iritis.

As to the particular action of calomel, Dr. Cross says, "It is said that mercury brings down, habitually, morbid stools. This is true in a particular sense only. If we are to understand by it that the alvine evacuations never assume a healthy appearance so long as the preparations of mercury are continued, the charge is wholly unfounded."

Mercury is the great rectifier of biliary derangement; it is known to possess more power in exciting and increasing the secretion of bile than any other article, and consequently when it is given it is natural to expect a greatly increased afflux of that fluid into the bowels. Now if the color of the stools be owing, as all must admit they are, to the presence of bile, it is very certain that when it is secreted in a larger quantity, or in a more concentrated state than usual, the dejections must be made darker than common, and this is often so remarkably the case that it renders them as black as those that are colored by sulphur or any of the preparations of steel.

Nor is it necessary for the chylopoetic organs to be in a morbid state for mercury to produce this effect. From the bowels of an individual in perfect health a scruple of calomel will cause to pass one or two stools of a much darker complexion than that which is natural to them, while a second dose will have no effect, but will bring down the contents of the bowels of a natural

color; and if the mineral should be persevered in, under such circumstances, they are apt to assume a preternaturally light hue, which sometimes goes on progressively increasing until they ultimately become nearly white. Now how are these various dejections under the use of calomel to be explained?

If the physiology of the liver, which we have endeavored to illustrate and explain, be admitted as correct, we shall encounter no serious difficulty in giving to this question a satisfactory response.

It must be recollected that we have endeavored to prove that the materials of the biliary secretions consist of the effete or refuse matter of the fluids and solids of the body. Assuming this as conceded, all will agree that when the liver is in the active discharge of its duty, the quantity of bile found in that organ and poured into the bowels must be very much in proportion to the amount of the elements of bile found floating in the abdominal venous circulation, and that in a state of perfect health they are not more abundant than is compatible with the complete integrity of the functions of the economy.

Several medicinal articles are believed to possess the power of increasing the biliary secretion, and of these, calomel is admitted to be the most certain and effectual. When, therefore, calomel or any other preparation of mercury is given to an individual in good health, the activity of the hepatic function is increased, and the elements of bile are separated, in a given time, in greater quantity than when the liver is under no such influence. This causes an augmented afflux of bile into the bowels, which gives rise to the dark hue which the alvine evacuations present after a first dose of calomel has been taken. The activity imparted to the liver by the first dose causes a rapid elimination of bile, and a consequent diminution of the quantity of effete matter in the venous circulation of the abdomen, and when a second is given on account of the deficiency of its elements, the amount of bile formed is much reduced, and the dejections assume a natural complexion.

Let the mineral, however, be persevered in; the liver thus

goaded will separate the elements of bile from the portal blood more rapidly than they will be thrown into the abdominal capillary veins by the intestinal absorbents, the consequence of which will be a gradual diminution of the quantity of bile formed, and a corresponding diminution of the rhubarb color of the stools, until at last they will become white.

The production of white stools has been mentioned as one of the effects of mercury, and they have been urged as an argument against the alleged peculiar efficacy of mercury to activate the biliary secretions. When they occur after the use of mercury and are disconnected with the jaundice, they should be regarded as resulting from an injudicious perseverance in a powerful medicine, which has caused a deficiency of the biliary principles.

This explanation of white stools is given by Dr. Thomson in his recent work on the Diseases of the Liver and Biliary Passages, though he undertakes not to explain how the deficiency of the biliary principles is produced.

By the author to whom reference has just been made it is asked, "May white stools occur in cases in which there is no jaundice, and if so, how can this phenomenon be accounted for? The only explanation which it seems possible to give of the occurrence is, that the blood is deficient in appropriate elements of the biliary principles, and that any fluid which the liver secretes is consequently deficient in these principles, for if the want of the due constitution of the bile depended, not on deficiency of proper materials in the blood, but simply on the liver being capable of separating them from that fluid, jaundice must arise from their retention as in the class of cases already referred to."

If there is any plausibility in the explanation above given, it is altogether probable that the varied appearances exhibited by the alvine dejections, and which have given rise to the assertion that mercury "habitually brings down morbid stools," results not necessarily from any morbid modification in the biliary secretion, but from the quantity of that fluid poured into the bowels; that although appearances seem to justify the allegation, there is evidently a period, during the administration of mercury, when the

amount of bile discharged into the bowels is neither excessive nor defective, and when, as a natural consequence, the fecal contents of the bowels present their natural character. This is, as we shall hereafter endeavor to show, the circumstance which indicates that mercury has been pushed far enough, and that to persevere in it longer is to run the risk of converting its sanitive into its morbid action. When, therefore, it is said that mercury brings down morbid stools, we are to understand that it causes the flow of an abundant and concentrated bile into the bowels, or that, from misapprehending its mode of action or the object to be attained by its administration, it has been enforced longer than is necessary or proper."

Two things must be observed in the above extract; the first is, that Dr. Cross has no knowledge of the physiological law, that in a state of perfect health the bile is formed from the food. Now my doctrine is, that in perfect health, when destructive metamorphosis is not at work, the food and effete blood are the only materials from which the liver separates and forms bile; and also that the large quantity of bile daily poured out into the duodenum in perfect health can only be accounted for in that way. This qualification has before been explained.

Second. Dr. Cross does not notice here, nor in any of his essays on the Function of the Liver or Intestinal Absorption, the source from which the urine is derived.

If, as he teaches, and as I believe, the bile is formed from effete matter, then we must account for the solid constituents found in the urine in some other way. I say they are formed from the food which is carried along in the current of arterial blood to the kidneys, just as the first bile is formed from the food brought to the liver through the portal veins.

Both of my assertions demand the most deliberate consideration of physiologists before they reject them.

I know nothing personally of the white or light-colored alvine discharges following the administration of calomel, spoken of by Dr. Thomson and Dr. Cross. I have no confidence in the supposed therapeutical discoveries for the relief of human mal-

adiers arrived at by experiments on the lower animals; nor have I much more confidence in conclusions drawn from experiments made by giving medicine to persons in good health. The instances referred to by Dr. Thomson were those in which the individuals were sick, and in whom destructive metamorphosis was at work. In these cases, I think, Dr. Cross's explanation is probably correct. But in the cases to which calomel was given, when the individuals were in perfect health, I suppose that the calomel given in repeated doses, so far interrupted the healthy functions of digestion as to prevent the liver from separating the usual quantity of bile from the food, without interrupting the general health so far as to set at work the process of destructive metamorphosis, by which effete matter would be thrown into the venous circulation. Besides, the individuals *in perfect health* selected for the experiments, were not, perhaps, after all, entirely so. Destructive metamorphosis may have been at work a short time before the experiments were made, and a smaller or larger quantity of effete matter would be in the venous circulation. The first dose of calomel stimulated the liver to remove this from the blood. The repeated doses only interrupted the functions of the liver in the secretion of bile. This is probably the correct explanation.

In another place Dr. Cross says, "We suppose that there is always a portion of refuse matter in the portal circulation that is compatible with health; that in certain diseases it is accumulated in undue quantity; and that when the alvine dejections, under the use of mercury, resume a natural character, all the refuse matter incompatible with health has been removed."

Dr. Cross again says, "More than once have we already alluded to the close connection that subsists between the liver and the skin, which by Dr. Johnson is denominated cutaneo-hepatic sympathy; and by which is understood a mutual dependence of function between these organs, which causes the particular state of one of them to produce a similar state of the other; that when there is an open state of the skin, there is at the same time an unobstructed state of the liver; that when the former

pours out perspiration freely, the latter discharges bile into the bowels abundantly, and *vice versa*."

Now to whatever principal recourse he had to explain this correspondence of action, no doubt can be felt by any physician who has practiced in warm latitudes, as to its actual existence. External heat is one of the principal causes of undue or excessive excitement of the skin, and it must therefore give rise to a copious afflux of bile. The quantity of bile mixed with the alvine dejections, being thus increased, a darker color than natural will be imparted to them, and they frequently become more acid than usual, causing more or less heat and sometimes pain during their dejection.

In accordance with a principle of easy and familiar application, which is that organs which have been exerted for a length of time become fatigued, perform their functions languidly and feebly, and are apt ultimately to fall into a state of torpor. In hot climates the skin is peculiarly exposed to this accident, and when it happens, the biliary overflux which accompanied free diaphoresis not only ceases, but the action of the liver is completely suspended, and then acute chylipoetic disease immediately ensues. This result is of much more frequent occurrence in southern latitudes than it otherwise would be.

Were it not that the influence of external heat is too commonly associated with the abundant use of distilled and fermented liquors, which greatly increase the effects of a high temperature, and consequently the function of the skin, already enfeebled by overexertion, is more easily checked by a more humid atmosphere, to which the inhabitants imprudently and without reflection expose themselves.

Now wherever there occurs, in those regions in which acute abdominal diseases prevail to much extent, a biliary overflux, as indicated by the alvine evacuations, the danger to be apprehended is of an entirely opposite state of the biliary organ—a torpid condition of the liver; and this very common result it should be the aim of the physician to prevent.

This is undoubtedly the indication which is to be fulfilled.

How is it to be accomplished? Evidently by such means as will achieve the twofold end in sustaining the liver in the performance of excessive duty, and of restoring the biliary secretions to their natural action. If mercury will increase the biliary secretion, it will of course sustain the action of the liver, and if the dejections become healthy in appearance under its use, it is of course by restoring the biliary secretions to their natural action.

That it excites the liver, is admitted by all, and that the stools assume a natural appearance under its use, we have already proved; therefore mercury is the article on which we should rely to diminish an overflux of bile.

The judicious and timely interposition of an occasional dose of calomel or pillulæ hydrarg. will soon bring the stools to a natural color, and then all solicitude on the subject will cease to be experienced.

Quietude, confinement within doors, and some gentle tonic, such as decoction of columbo, will restore the previously over-excited organs to a normal condition. In addition to the above and in agreement with it, I know that the excessive drinking of alcoholic or malt liquors often stimulates the liver to an increased secretion and pouring out of bile, so that there is a sort of bilious diarrhea, which is sometimes followed by torpidity of the liver and costiveness. There are no more certain and effectual remedies for this last condition than a thorough emesis and a scruple dose of calomel. Sometimes the emetic itself will be sufficient. It arouses both the liver and skin to healthy action.

Some very distinguished physicians have attributed green stools to the action of calomel. Upon this subject Dr. Cross remarks: "During infancy it is not uncommon for green stools to pass from the bowels that are copious, thin, mixed sometimes with mucous flocculi that resemble closely-cropped grass or spinach. Alvine dejections, more or less green in appearance, are sometimes passed by adults, but they are not so liquid as those which come from children. In regard to the source of such dejections much discrepancy of opinion exists among physiologists. That this should not happen must be a matter of surprise to

every thinking mind capable of taking a comprehensive view of the subject. Cystic bile is green or black, and therefore green stools should be ascribed to the liver, unless the most positive proof can be adduced that they come from some other source."

Cystic bile, as we have said, is of various shades of green or black, the color of it depending upon the length of time it has been retained in the gall-bladder, and when black, if diluted with water it becomes green.

It must be recollected that bile is not green when it is secreted, but assumes that color during its transit to and retention in the gall-bladder, and it is therefore an acquired property, which results from changes which are produced in it after its exclusion from the sphere of vitality.

Green stools, consequently, coming, as they do, and as we shall presently prove, from the liver, can not be looked upon as even morbid in any other light than as being formed of materials more disposed than in health to run into those changes upon which the sensible properties of cystic bile depend. This, we say, because either from the influence of disease or from the action of medicines, as is proved by the copiousness and frequency of green stools, the biliary secretion remains but a short time in, if indeed it is permitted in all cases to pass at all into, the gall-bladder.

But let us examine the explanations that have been given of stools under consideration. As we have said, they have been ascribed to the acidity of the contents of the *prima viæ*. This supposition is rendered wholly incredible by the fact to which Abernethy and Ayre testify; and which is, that in those cases that proved fatal during the passage of green stools, precisely the same fluid was found in the bile-ducts and gall-bladder. In several fatal instances of hydrocephalus this observation we have been able to verify. . . .

In a natural state the mucous membrane of the bowels forms no such product from the blood, and when irritated we have no proof that its secretion is capable of undergoing any such change. Did Professor Graves or any other pathologist ever find the con-

tents of the bowels green without finding a similar color characterizing the contents of the gall-ducts and gall-bladder? No, we are sure he never did.

Ammonia, soda, potassa, magnesia, and creta preparata have often been given to children with green stools, and we have never seen such articles produce any beneficial change in them, while a few doses of calomel and prepared chalk have achieved in that respect all that we desired.

I could verify the correctness of this last statement by a report of many cases of infants and young children, where these green stools appeared, which were promptly and decidedly relieved by the administration of calomel in sufficiently large doses. In the summer complaints of children and of cholera infantum, these green stools are among the most unfavorable symptoms, and they are often accompanied by irritability of the stomach with frequent vomiting. In such cases it will often be found necessary to give scruple doses of calomel. The reader need not be alarmed, for there is no remedy which produces quietude and an entire subsidence of both vomiting and purging so promptly. I will refer to the use of calomel in these complaints again.

Dr. Cross says, "Dr. Mills, in his work on Disorders of the Brain, records a fatal case of diseased brain in the treatment of which not a grain of calomel had been employed, yet throughout its whole course green stools were passed by the bowels. He says, during the illness the feces were greenish, an appearance commonly imputed to the use of calomel, but which is more properly referable to the condition of the bile; green bile was poured in the gall-bladder and ducts, whence it was traced to the small and large intestines every where tinging their contents with a green of a deeper or lighter shade, according to the quantity and nature of the matter with which it was mixed.

"In two cases of hydrocephalus which proved fatal, while green stools were being voided the secretions found in the gall-bladder and ducts were very similar to, if not identical with, that which was found in the intestines and which had passed from them several days before death.

“So entirely exempt is calomel, according to our observation, from having any tendency to produce the variety of stools under consideration, that we have for many years regarded it as the safest and most certain remedy that can be given for their correction.

“Confiding in the truth of the practical views we have announced on the subject, we would, in addition, venture to admonish the reader that when, under the use of calomel, green or spinach-colored dejections change their character for the better, he should not expect them to resume at once the bright yellow complexion characteristic of healthy stools, for they will become of a much darker green and sometimes they will be observed to be nearly black, after which, if the mineral is properly persevered in, they will become of a less and less dark color, and in a few days they will ultimately exhibit their proper hue.”

When there is decided hepatic derangement, it is useless to expect its perfect rectification from one, two, or three doses of calomel. It is often very slow to take hold of that organ, and we frequently find it necessary to continue it for days in succession, before such an impression can be made on it as is desirable.”

“The dark or black evacuations, which calomel rarely fails to bring down, have been, like those that are green, ascribed to the chemical action of the liver upon the intestinal contents. This opinion is daily rebuked by observation and experience. If true, they would become darker and darker under its use, while we know they present, in succession, various shades of color and ultimately resume the natural complexion.

“In disease the alvine evacuations often cause a hot or smarting sensation during their passage per anum. This sensation is sometimes so intense as to be very painful, and the contents of the bowels are often so acrid as to produce excoriation. This acridness has been ascribed to the medicines that are employed. This is true, but not in the way it is commonly explained. It is supposed that the acrid and drastic qualities of certain medicines give rise to the pain and excoriation just spoken of. Our observation lends no support to this opinion. Unless the lower

part of the intestinal tube is irritated or inflamed, as in dysentery, we know of no medicine that renders the contents of the bowels acrid, except those that are known to possess the power of invigorating the action of the liver; nor do we ever find the stools acrid, except when there is unequivocal proof of abundant biliary secretion. Then they produce heat, smarting, and sometimes excoriation. Such sensations are never experienced in those states of the diseased system which are distinguished by the unequivocal evidences of an inert liver; and therefore they may be fairly attributed to an unobstructed condition of that organ."

I have repeatedly taken calomel in scruple doses; I have taken cathartic pills composed of calomel, socotrin, aloes, and ipecacuanha, and pills composed of the same medicines and rhubarb; I have taken pills composed of calomel and the compound extract of colocynth; I have noticed the stools, and sometimes found them muddy, sometimes yellowish, sometimes green or greenish, and sometimes very dark or nearly black. I have also felt the burning sensations spoken of by Dr. Cross in the extract just quoted. I have always felt relief from such discharges; and I have also noticed that as convalescence advanced the alvine discharges assumed more and more a rhubarb color, with the odor and consistence peculiar to health.

The physician who would use calomel understandingly must first acquaint himself with what constitutes healthy stools—as to their color, consistence, and odor. I have verified these observations through many years' practice. I am satisfied that physicians and medical writers who object so much to the use of calomel have never tried it sufficiently to know any thing about its medical virtues. No modern work on the practice of medicine or on therapeutics gives such instructions to the young physician as would enable him to know when and how to use it. The common error is to give it in too small quantities.

The source of another error is that the books do not teach what sort of stools calomel ought to bring down. In all forms of disease in which there is loss of weight, destructive metamorphosis is at work, and a smaller or larger quantity of effete matter,

proportionate to the loss of weight, is thrown off. This effete matter, passes first into the venous circulation, and has to pass through the liver or lungs, or through both, before it enters the arterial circulation. Independent of the remote cause of the particular affection, this effete matter is itself a disturbing cause, involving an interruption of the healthy process of digestion, thus preventing the proper conversion of food into blood. This disturbs the healthy functions of every organ in the body.

It is the peculiar function of the liver to remove this effete matter and not only to convert it into bile, but also to pour out this bile into the duodenum. In some forms of disease, as the exanthemata, the liver generally performs the office just referred to sufficiently well; and there is no necessity for giving any medicine to excite it to increased action. The only exception to this is when the exanthemata are of the congestive form, which is the most fatal form. In this case it may be necessary to give calomel. Ordinarily I do not give it or any other cathartic in such affections.

I have before said that except in the forming stage of typhoid fever I do not give calomel. I consider it to be a very doubtful remedy in cases of dysentery, unless there be very evident signs of congestion of the liver. But in miasmatic fevers, in cholera infantum, and in Asiatic cholera, I believe it to be by far the most reliable remedy. It is also a remedy of incalculable value in many forms of chronic disease—in all those where there is evidently congestion of the venous cavities.

In those chronic affections in which these congestions exist I have found calomel combined with ipecacuanha, aloes, rhubarb, and a compound extract of colocynth, or with one or two of them, to be preferable to calomel alone, though sometimes a scruple dose of the latter will be necessary. I believe calomel to be one of the most reliable remedies in pneumonia. A large amount of effete matter is often thrown into the venous circulation in that complaint. The lungs are unable to purify the blood, as in health, and the whole of that task devolves upon the liver. That organ must be roused to complete action; and sometimes the relief will

be manifest before the bile passes off in the alvine discharges. I could cite many remarkable cures of patients who have been given up to die before I was called to see them.

I will only give one. It was the case of my fifth son. He was four months old when he had a terribly severe attack of pneumonia. One of our best physicians was attending the child with me. After he had been ill three or four days and was growing worse all the time, this physician called in the evening and said he was dying. A few minutes after he left my house a neighbor asked him how the baby was. He replied that he supposed he was dead, as he was breathing his last when he left the house. Knowing that calomel exhilarated me like champagne wine, I determined to try it on my child, as I did not agree with the doctor that he was dying, and knew that he could still swallow. I gave him twenty grains of calomel. It aroused him in less than an hour. This was about 7 o'clock P. M. About day-break next morning finding that the reaction was subsiding, I gave him twenty grains more, although the other had not operated. In the course of the forenoon black bile flowed freely from his bowels. After this the bilious discharges were less dark, and they soon became of a rhubarb-yellow. He took little or no medicine after this, and convalesced rapidly. He is now one of the stoutest young men in Colorado, being in the twenty-second year of his age.

Calomel given in scruple or large doses operates very slowly; and it is frequently necessary to give some other cathartic to work it off. Given in these doses it does not prostrate, as it sometimes does when given in small doses frequently repeated.

Many physicians who give calomel in small doses often repeated, give more to a patient within a given time than I do. It is a very great mistake to suppose that calomel given in scruple doses, even to infants, will salivate. In over forty-three years' practice I do not think I have salivated over a dozen or at most twenty patients, and not one of them was an infant or a young child; and in not a single instance have ever had cause to regret giving calomel in these large doses.

In cholera infantum, where the bowels are continually running off, and where there is so much irritability of the stomach that every thing swallowed is immediately vomited up, a scruple dose of calomel will act like a charm, quieting immediately both stomach and bowels, and producing refreshing sleep. When the child wakes he will have a healthy appetite, and the food given him, or his mother's milk, will be digested without interruption. The dose of calomel, both among the young and the old, must be determined by the physician to suit each case.

I have spoken of large doses, not only to show that they may be given, but that it is often necessary to give them. To infants and young children I often give calomel in half-grain and grain doses; and to adults I sometimes give it in from five- to ten-grain doses; but when I give calomel to adults, in small quantities, I prefer uniting it with ipecac and some other cathartic.

But lest it may be supposed that I give nothing but calomel or some other form of cathartic medicine with emetics, I must here say that the practical part of this book is to explain the uses of those particular medicines. In some forms of disease I never think of giving a cathartic. I will refer to one of them, a very severe malady and one which is too often fatal. I allude to diphtheria.

Nineteen years ago, before I lost my eyesight, I treated twenty-six cases of diphtheria at the St. Catherine's Academy in this city. Three of the cases were very serious ones, the others were milder. The severe cases were the first attacked, and I did not see them until the disease had made some advance. The other cases I saw when the first indisposition manifested itself.

I had studied the disease as reported in the medical journals for some twenty years, and saw no good reason for giving any internal medicine. I directed the sisters to require the girls and young ladies to drink freely of catawba wine or good whisky toddy, whichever they preferred. I also directed that each patient should have a plate of lumps of ice by her and that she should keep a lump of it almost continually in the mouth. If this was not sufficient to control the diphtheritic affections of

the throat, I had a small towel, wrung out of ice-water and folded up, applied to the throat from ear to ear. This was covered by a piece of dry flannel. The towel was frequently rewet in ice-water. They all had nutritious food as they wanted it. They all used a gargle of chlorinated soda one ounce, tincture of myrrh one ounce, water six ounces. To one of the girls I gave a dose of castor oil; to another I gave a dose of castor oil and some sweet spirits of niter to excite the kidneys to action. I will describe this case more particularly. She was a young lady in her fifteenth year, but remarkably large for her age. The tonsils and soft palate were very much swollen. The diphtheritic exudations were extensive. The glands about her neck externally were very much swollen. She had a very high fever with a bounding pulse. The skin was excessively hot to the touch. Her whole person was as red as a coal of fire. She was wild with delirium. I had a tub filled with water and lumps of ice. Two sheets were put into it. The sisters stripped her and applied one of these sheets to her person from her chin to her feet. The steam rose from her in a cloud. She became quiet immediately. But as soon as the sheet became warm she began to rave again. The other sheet was then applied. This was continued for an hour, when the fever subsided, and there was no further trouble in her case.

Twenty-four of these girls did not take a dose of medicine of any sort. They all recovered rapidly. I do not doubt but that others of them would have been bad cases had they not drunk the wine or toddy freely, and held the ice in their mouths.

I afterward treated one of my sons, then in his fifth year, in the same way. When the wet towel around his throat would begin to get warm, he would say, "Father, put the towel in the ice, it makes me feel good when it is cold." He was soon entirely well.

After this I saw three cases of diphtheria—children in the same family. I was called in consultation. They were then in the blue or congestive stage. The physician in attendance told me they had gone very rapidly into this stage. Like similar

cases of scarlet fever, of Asiatic cholera, and of congestive fever, the vital forces were too far overpowered for any thing to be done.

I prescribe no medicinal drug. With a few exceptions there are no specifics. There are other valuable cathartics besides those which I have named. Among saline cathartics I consider the salts made at Crab Orchard Springs, Kentucky, to be by far the best. They contain other ingredients besides the sulphate of magnesia. Dr. Robert Peter's analysis of these salts can be found in the Geological Survey of Kentucky by Dr. David Dale Owen. Opium, veratria, belladonna, aconite, and many other articles of this class, are valuable; but, for the most part, they effect very little benefit, unless the secretions and excretions are corrected, as explained in former chapters. The preparations of iodine are also valuable; but they are too frequently given in doses unnecessarily large. The medical journals are full of reports of cases in which physicians appear to boast of the enormous doses they give of this, that, or another drug; of any thing, it would seem, except calomel. This applies particularly to obstetrical cases, and especially to those in which puerperal convulsions occur.

In these last, hundreds of women, in my opinion, have been killed by chloroform and preparations of opium hyperdermically administered or otherwise. Dr. David Keller, of Paris, Ky., told me that he had found teaspoonful doses of calomel to relieve puerperal convulsions in quite a number of cases. He has had a large obstetrical practice, and has every confidence in the efficacy of his remedy. I have had no experience in such cases which is worth relating, but I have all confidence in Dr. Keller's statements.

A once distinguished Kentucky physician, Dr. Lewis Marshall, used to say that any man was a fool who required over two hours to deliver a full course of lectures on obstetrics. He was an extravagant man to talk; but he had particular reference to the duties of the accoucheur, and meant that the woman's

general health should be looked after beforehand, and that the secretions and excretions should be properly regulated. When this is done, very many troubles, some of them fatal, will be avoided.

Mr. Abernethy acquired great fame in London, and it has extended to the United States, by curing surgical troubles through constitutional remedies. Nowadays in consequence of the scientific (so-called) errors, which I have endeavored to expose, it is too often the case that surgical methods are resorted to in the removal of maladies which were more constitutional than local. This applies particularly to gynecology and the lesions of the urino-genital system, and also to diseases of the eye. There is no department in the practice of medicine in which improvements so great and beneficial have been made than in the maladies just referred to. But in these cases too little attention is often paid to the general health of the individual.

A gentleman applied to me a few years ago to treat his eyes. I told him that I was surprised that he came to me for that purpose; as I was satisfied that I would not have been as nearly blind as I am had I been able to look at my own eyes. He said his general health was out of sorts. He said he had been to consult one of the most distinguished oculists in the United States, who had told him it would be necessary to take out his left eye to save his right one. This he was unwilling to submit to. I told him that I would undertake to correct his general health. I gave him pills composed of calomel, ipecac, aloes, and rhubarb, in equal quantities. He took from six to ten grains of this mixture every night or every other night. The improvement of his general health was rapid. He said that all of his friends who met him noticed it. His eyesight was also improved. Having business in the city where the distinguished oculist lived, he called to see him and his partner or assistant.

The distinguished oculist was abroad, but the other gentleman noticed the improvement, not only of his general health but also of his eyes, and said, "We made a very great mistake when we recommended that your left eye should be removed to

save the right one. We would not take it out now for any consideration."

There are some maladies, cancer and syphilis for instance, which seem to be compatible (for a time) with a moderate amount of general good health; that is to say, the digestive system is able to convert food into blood, and to sustain the ordinary powers of life. This applies also, as is well known, to cases of insanity, idiotcy, and imbecility. The asylums all present cases in which, but for the troubles referred to, the individuals are physically well.

It is in this latter class of cases that medicines, which act particularly upon the nervous system, are most valuable. But they are too often given in such cases without sufficient attention being paid to the secretions and excretions.

As to the use of mercury in the treatment of syphilis, Dr. E. L. Keyes, of New York, says, "In 1874 Wilbouchewitch, making use of the microscope and a special instrument for blood-counting, studied the blood of syphilitic hospital patients who were taking mercury. He concluded from his observations that small doses of mercury at first increased the number of red cells in the blood, but, if long continued, produced the same results as large doses in animals, namely, a diminution of the red cells, diarrhea, etc.

"It is well known also that Grassi and Ricord, by chemical methods, determined that the use of mercury early in syphilis diminished the number of red cells relatively to the whole mass of blood.

"Here, then, was the problem how to reconcile such seemingly conflicting results arrived at from the same premises.

"The solution became easy when it was recognized that the premises were not the same. Grassi did not even pretend to use small doses of mercury, but pushed the drug to its toxic effect, and then found that it was debilitating, a result which all must admit.

"Wilbouchewitch also overdosed his patients, though to a less degree, and he does not seem to be aware of it, although his

patients lost weight and got diarrhea. Of his ten cases, five got over one half grain of corrosive sublimate a day, the rest a grain and a half of protoiodide; surely in neither case a very small dose for a patient on hospital diet.

“My own experiments with the hematimeter, using minute doses of mercury upon healthy and upon syphilitic individuals, I think have proved beyond question, that mercury in minute doses is tonic in all cases where it can be digested, in syphilis or out of it, continued for a short or a long (over three years) time.”

I do not discredit Dr. Keyes's observations, but I would not follow his practice. If mercury increases the number of red cells in the blood it is not because, in my opinion, it is digested, as Dr. Keyes expresses it, but because the general health has been improved by the administration of the drug.

The mercury only removes the difficulties which obstructed the generation of red cells. In that sense, and in that only, is it a tonic. It is not a constituent part of the blood, as is iron. Two of the most distinguished of German physicians, Drs. Niemeyer and Liebermeister, class mercury, I believe in all its forms, among the antiseptics. I believe that scientific physicians generally agree with them.

Now, if it is an antiseptic, I can not see how it can be classed among digestible articles.

The German physicians, as well as all others in Christendom who assume to call themselves scientific, adopt the theories of M. Claude Bernard and Dr. J. Hughes Bennett, as to the functions of the liver, and the sources and uses of the bile.

Denying that the bile is an excrementitious fluid, and also denying that calomel has any power to excite or increase its secretion in the liver, while they are bound to admit that patients are greatly benefited from its administration, they have to account in some way for its therapeutical efficacy, and so they call it an antiseptic.

I have not adopted the Little Bugs theory of disease; it may be scientific, but it belongs too closely to the infinitesimals of

Hahnemann and to the problem of St. Thomas Aquinas, for my comprehension.

I would not exclude the Little Bugs altogether, but as to the origin of fevers, I rather agree with Dr. Daniel Vaughan. If my views, as to constructive assimilation and destructive metamorphosis; as to the functions of the liver and the uses of the bile; as to effete matter; and as to the therapeutical efficacy of calomel, are correct, then it will not be necessary to say that mercury is digested or that it is an antiseptic.

I use iron in its various preparations in the treatment of different maladies. Having had ague-and-fever so much myself, and having had a good deal of experience in the treatment of it, I have paid particular attention to the best method of cure. I have given my prescriptions to many physicians in Kentucky and other states.

As I know that two men are selling my remedies under their own name and for their own exclusive benefit in half a dozen states on the Ohio and Mississippi rivers, I will here give them. The first I call "ague pills or powders." They are composed of prussiate of iron sixty grains, sulphate of quinine sixty grains, capsicum thirty grains, soc. aloes thirty grains, arsenious acid one grain, to be made into thirty pills or powders. I commence from eight to ten hours before the expected chill and give a pill or powder every hour till six doses are taken. This will almost always prevent the chill or ague. On the next day I commence at the same time and give a pill or powder once every two hours until four doses are taken. On the third and all subsequent days begin at the same time and give a pill or powder every three hours until four doses are taken. I also use what I call cathartic pills. They are composed of sulphate of iron twenty grains, soc. aloes twenty grains, calomel twenty grains, to be made into twelve pills. I give from one to two of these pills every night or every other night until all are taken. If this course is followed it will very rarely happen that there will be any recurrence of this troublesome malady.

Quinine is an antiperiodic, and will stop chills, but it will not

prevent their return, unless the secretions and excretions are attended to. I do not believe in giving quinine in toxic doses. Every physician who has had an extensive private practice knows that a majority of the cases which he is called to see would recover without medicine if carefully and properly nursed and judiciously fed.

A little medicine may sometimes be given to such cases with advantage. It satisfies the mind of the patient or of his friends, and the individual will probably recover in a shorter time. It is only the occasional case which demands the skill of the *physician*. What he thinks the case requires should be done promptly and boldly.

When an individual faints, when he or she has an epileptic fit, or is struck down by apoplexy, it is best to do nothing, except to place the patient in a recumbent posture, to see that their clothes are loosened, and that they have fresh air. The boldness here necessary is to quiet alarmed and excited friends and bystanders, and to prevent them from doing what might be hurtful.

Sometimes a physician is called to a case. He examines the patient carefully, and says "we must wait, there is nothing to be done at present." It is no doubt the best advice. Sometimes he is called to a case which requires a prompt and powerful remedy; then he should not hesitate to give or to use the remedy. To know how and when to do this is what constitutes the *good physician*.

Fevers and inflammations, and congestive affections, like cholera infantum and Asiatic cholera, kill a majority of the human race; and a very large proportion of that majority is among those who were in good health at the time of their seizure—young men in the prime and vigor of life, and young women in the bloom and beauty of their loveliness. To know how to treat these acute maladies successfully the physician must understand the principles I have endeavored to explain in former chapters.

I have pulled down the temple of medical science erected by the philosophers of the nineteenth century; but I am not an

Erostratus; I have laid the foundation for a nobler and more enduring temple in its stead.

Kepler explained the laws which govern the solar system. I have undertaken to explain the laws which govern the animal economy, both in health and in disease.

I acknowledge my obligations to Drs. Flint and Dalton—copies of whose books I borrowed—for their learned works on physiology. I cheerfully recommend these works to all medical students, and to young physicians, for I am convinced that a successful practice must depend upon a correct and thorough knowledge of physiology.

Having concluded what I have to say on general principles, the following and concluding chapters will be on chronic diseases and Asiatic cholera.

CHAPTER XIV.

CHRONIC DISEASES.

“Achilles’ wrath, to Greece the direful spring of woes unnumbered,” did not send as many mortals beyond the Styx as have false and absurd medical theories, which, in their day and generation, were supposed to be scientific. Boerhaave was the most distinguished physician of his day; so much so that the Emperor of China consulted him. He was a man of great learning. His theory abolished the use of cold water in Europe in the treatment of fevers for over a century; and many parts of Christendom are to-day under its influence.

Thanks to Priessnitz, the people of Europe and the United States now have a better knowledge of the value of cold water in treatment of fevers and other maladies. I say, advisedly, thanks to Priessnitz, because Dr. Currie’s doctrines were never known, or if known, were not understood when the hydropathic system of the German peasant was established. And to-day both the practice of Currie and Priessnitz are likely to go into disuse because of another theory—viz. that high temperature, that is a temperature above the standard of health, is the cause of death in fever cases; which cause has to be controlled by one of two methods—viz. toxic doses of quinine, that is, from thirty to sixty grains at a dose, or the immersion of the patient into a cold bath every two hours, whether his teeth chatter or he shivers as if from an ague or not.

I say this is the new theory, which is likely to abolish the practice of both Currie and Priessnitz, among those physicians

who assume to call themselves scientific. The authors of this theory give fanciful and imaginary explanations as to the cause of the high temperature.

When Claude Bernard discovered, or supposed that he discovered, that the particular and especial functions of the liver were to produce or create sugar and fat; and when he, or somebody else, thought that they had discovered that another function of the liver was to form urea out of constituent parts of healthy blood, then it was altogether unscientific for a medical man to pay any attention to the bile-producing abilities of the liver. Besides it had been discovered by scientific physiologists that the bile was not an excrementitious substance; but that it was incrementitious, and that it was all reabsorbed except the cholesterolin.

The logic of science forced physicians, who assume to be learned, to the conclusion that medicines called cholagogues were altogether useless, if not injurious. Hence the Surgeon-general of the United States Army, who is to-day one of the most learned and distinguished physicians in the city of New York, ordered, or attempted to order, that mercury in all its forms, and particularly calomel, should be excluded from the medical army-chests during our late tremendous war. I say tremendous war, for there were over four millions of men engaged in it.

Had any severe epidemic attacked either the Federal or Confederate armies (and it is a remarkable fact that no epidemic attacked either, though the war lasted over four years), and had Dr. Hammond's scientific order been carried out, it is more than probable that science would have killed many more Federal soldiers than did the rebel cannon-balls and bullets.

After this came the report of Dr. J. Hughes Bennett. Had the Surgeon-general of the United States Army had this report on which to stand, nothing less than the common sense of President Lincoln could have prevented the exclusion of calomel from the medical army-chests; for every medical man within the Federal lines, who pretended to be scientific, would have fully sustained Dr. Hammond.

It is perfectly marvelous how few men think for themselves.

The practice of medicine never was at any period of the world's history more thoroughly empirical than it is to-day. I do not refer to the practice of surgery. Thanks to John Hunter, that is better understood.

The medical schools teach no principles which their students can understand, hence they have to follow authorities.

While the theories of Claude Bernard and J. Hughes Bennett were considered to be demonstrated science, a young physician just from Philadelphia, New York, London, or Paris, would as soon have thought of looking into the palm of the hand to tell the condition of his patient as to look at the stools.

But another authority has loomed up. I refer to Charles Murchison, M. D., LL. D., F. R. S. He is a distinguished London practitioner; and is no doubt deservedly distinguished. His work on the Functional Derangements of the Liver could not have appeared at a better time.

The people throughout this country and Europe still continue to discover, as often as they are sick, that a dose of calomel or blue-mass does relieve them of their ailments, science to the contrary notwithstanding; and they so tell their medical practitioners or family physicians.

But as these common people know nothing about science, let their feelings tell them what they might, the learned physicians required an authority, and Dr. Murchison came to their rescue; though many physicians still cling to science, let popular experience tell what it may. Dr. Murchison thus explains the case:

“*Cholagogues*.—Among these remedies mercury and its preparations hold a preeminent place. At the present day mercury has lost much of its former reputation as a cholagogue and alterative, and there is much difference of opinion as to its power over the liver. The practical physician gives a dose of calomel, finds the quantity of bile in the motions greatly increased, and his patient's state much improved; and he argues that the liver has been stimulated by the mercury to an increased secretion of bile, and that to this cause his patient's improvement must be

ascribed. The physiologist, on the other hand, ties the common bile-duct in one of the lower animals, produces a fistulous opening into the gall-bladder, and then finds that calomel has no effect on, if it does not diminish, the amount of bile that drains away through the fistula. It may interest some who are present if I refer briefly to the principal of these experiments.

“Kölliker and Müller, in 1855, tried the effects of calomel upon the secretion of bile in a dog with a biliary fistula. The results were somewhat contradictory.

“Once the bile seemed to be increased, and twice it seemed to be diminished by the administration of calomel.

“Of four experiments made in 1858 on a dog with a biliary fistula, Dr. George Scott found that in all the administration of large doses of calomel was followed by a diminution of fluid bile and of bile solids.

“In the same year (1858) Dr. Mosler made similar experiments upon two dogs with biliary fistula. The administration of calomel was not followed by any increase of bile, nor could mercury be detected in the biliary secretion.

“Ten years later (1868) a committee of the British Medical Association, with Professor Hughes Bennett, of Edinburgh, as chairman, made a number of similar experiments on dogs, and came to the conclusion that mercury did not increase the flow of bile, but rather diminished it.

“Next in order (1873) came the experiments of Dr. Rohrig, of Kreuznach, made in the Pathological Institute of Vienna. He found that although large doses of calomel did seem to increase somewhat the secretion of bile, its power to do so was inferior to croton oil, colocynth, jalap, aloes, rhubarb, senna, and sulphate of magnesia, the cholagogue power of these drugs diminishing very much in the order in which they have now been enumerated, and calomel standing at the bottom of the scale.

“The most recent experiments are those of Professor Rutherford and M. Vignal, on four different dogs during fasting. In three the secretion of bile was diminished, and in one it was

ascertained that not only the total quantity, but the percentage of solids, was reduced. In the fourth case the quantity of bile was increased, but there were reasons for believing that the increase was not due to the calomel.

“The results of experiments upon the lower animals have added greatly to the discredit previously thrown upon mercury by its failure, when brought to the test of accurate clinical observation, to absorb plastic lymph in most forms of inflammation; and some eminent physicians are even of the opinion that mercury and its preparations ought to be erased from our pharmacopeia. On the other hand, it has been fairly objected that the results of experiments with mercury upon dogs do not warrant conclusions as to its effects upon man; and even granting that in man mercury does not increase the quantity of bile secreted by the liver in health, it does not follow that in disease there may not be some condition adverse to the formation of bile which mercury may have the power of removing. Much, however, of the difference of opinion between physiologists and the practical physician may be reconciled by keeping in mind the osmotic circulation, to which I referred in my first lecture (p. 38), as constantly going on between the intestinal contents and the blood. A large part of the bile secreted by the liver and thrown into the bowel is constantly being reabsorbed to reach the liver again; and accordingly when the common bile-duct is tied and a fistulous opening into the gall-bladder established, the quantity of bile which escapes through the fistulous opening immediately after the operation is much greater than at any time subsequently (Schiff). Mercury and allied purgatives produce bilious stools by irritating the upper part of the bowel and sweeping on the bile before there is time for its reabsorption.

“The fact of mercury standing at the bottom of the scale of cholagogues in Rohrig’s experiments is accounted for by its surpassing other cholagogues in this property; for of course the larger the quantity of bile that is swept down the bowel, the less is reabsorbed, and the less escapes from a biliary fistula. That mercury does act especially upon the duodenum, is proved not

merely by the large flow of bile which follows its action, but by the fact discovered by Radziejewski, that leucin and tyrosin, which are products of pancreatic digestion, under ordinary circumstances decomposed in the bowel, appear in the feces after the administration of mercurials. It would appear then that mercury, by increasing the elimination of bile and lessening the amount of bile and of other products of disintegrated albumen circulating with it in the portal blood, is, after all, a true cholagogue, relieving a loaded liver far more effectually than if it acted merely by stimulating the liver to increased secretion, as was formerly believed, and as some authorities still maintain; for in this case it might be expected to increase, instead of diminish, hepatic congestion. It is not impossible also that the irritation of the duodenum by calomel and other purgatives may be reflected by the gall-bladder, and cause it to contract and discharge its contents, and thus account in part for the increased quantity of bile in the stools.

“There are likewise, I believe, grounds for believing that apart from its increasing the discharge of bile from the bowel, mercury exerts a beneficial action in many functional derangements of the liver, in whatever way this is to be explained. Patients of the greatest intelligence suffering from hepatic disorders constantly declare that they derive benefit from occasional or repeated doses of mercurials which no other medicine or treatment confers; and the skepticism of the most doubting physician would, I believe, be removed should he unfortunately find it necessary to test the truth of their statements in his own person.” (Pages 175 to 180.)

Dr. Murchison's work is really valuable in the practice which he prescribes; though he holds on to science with a full hand and firm grasp, while he touches daily observations and practical experience with the tips of the fingers of his other hand; and even then he has to call in osmosis and exosmosis to relieve him of the dilemma. He teaches that the bile is partly excrementitious; though, as he admits, physiological science denies the fact.

As to the method by which mercury acts, he says, "It is not impossible that the good effects of mercury on the liver, and in some forms of inflammation, may be due to its property of promoting disintegration. Mercury appears to have the power of rendering effused fibrin less cohesive and more easily removed by absorption than it otherwise would be."

Among the chronic diseases for which he prescribes calomel or blue-mass, I will mention lithemia and the various forms of dyspepsia, with which it is very often associated. He says:

"From what has been already stated, it is clear that the kidneys and the liver are intimately connected in their functions, the main object of the kidneys being to eliminate certain products which are in great part secreted in the liver."

He speaks of "diseases and symptoms resulting from the abnormal disintegration of albuminous matter in the liver"; and of fibrin he says, "It is not improbable that the large quantity of fibrin found in the blood of acute rheumatism may result from fibrin not being destroyed in the liver to the proper extent."

Again he says:

"*Pyrexia*.—The liver is one of the few parts of the body which does not waste during the fever. On the contrary, it becomes enlarged and congested, while its gland-cells are swollen out with minute albuminous granules; and it is well known that these changes are attended by an increased disintegration of albuminous matter and an increased production of urea and less oxidized products."

It will be clearly seen from these last quotations, that Dr. Murchison teaches that urea and other less oxidized products are created in the liver; and that it is a special function of the kidneys to remove these products; and that when other less oxidized products than urea are formed in the liver, then more difficult work is thrown upon the kidneys.

I do not intend again to go over what I have so extensively discussed as to the relation between the liver and kidneys in former chapters; but I must call the attention of the reader to the consideration of some facts which always occur in chronic

diseases, and which facts have particular reference to the functions of the liver in pathological states of the system.

If I understand Dr. Murchison and other physiologists, they all teach that the bile is formed from disintegrated albumen and, perhaps, fibrin. These are constituent parts of pure blood.

I have before spoken of the lungs and liver being compensatory organs.

The pulmonary artery conveys from the right side of the heart to the lungs, not only the venous blood proper, but also a large amount of lymph, which is poured into the descending vena cava from the thoracic duct. The pulmonary veins convey to the left side of the heart a smaller quantity of fluid, called blood, than is carried to the lungs by the pulmonary artery. It is minus the quantity of carbonic acid and watery vapor expired. These are unquestionably excrementitious.

Now it is also known that the hepatic veins convey to the vena cava a smaller quantity of blood than is brought to the liver by the great portal vein and the hepatic artery. It is minus the bile which was separated from the fluid, called blood, passing through the liver.

Now physiologists teach that the bile is all reabsorbed except the cholesterin; and that this is a constant fact. If they are correct, I demand that they shall explain why less blood flows from the liver than enters it. If a boy takes a bag of corn to mill, he gets a quantity of meal minus the toll taken out by the miller. Now, if the miller pours the toll back into the hopper, why does not the boy get all the meal which the corn could make? The bile is the toll taken from the blood by the liver. I maintain that it is altogether and entirely excrementitious; as much so as the carbonic acid and the watery vapor thrown off from the lungs. I have said that constructive assimilation and destructive metamorphosis, or the separation of effete matter from the living organized tissues, do not go on in an animal body at one and the same time; and that, in a state of perfect health, the separation of effete matter from the living tissues does not occur at all. But I teach that this separation of effete

matter does occur in all acute diseases, of any duration; and that it always occurs in chronic diseases, except those (like some forms of insanity) in which vegetative or organic functions are not disturbed. It does occur in all of those forms of disease treated of by Dr. Murchison in his recent work.

Let us now consider the character of the fluid which courses through the great portal vein on its way to the liver. It has three distinct characters, all unlike each other. The first is the food, under its several forms, which is absorbed by the veins from the stomach and intestines. That portion of it is mixed up with the saliva, the gastric, the pancreatic, and the intestinal juices.

The second form is composed of blood returning from all the abdominal organs. Arterial blood was carried to those organs, and it parted with what went to make up the gastric, pancreatic, and intestinal juices. This venous blood, then, is to that extent effete. But in these pathological states the capillary vessels take up from the living tissues effete matter, the product of destructive metamorphosis. This effete matter is emptied into the veins, which return it to the liver, and they are to that extent filled with effete matter.

Now the third constituent part of the fluid found in the great portal vein is, in health, rejuvenated blood; that is, an increased quantity of fibrin, and albumen, and white blood corpuscles, coming from the spleen. This, however, does not occur in diseased conditions to any extent, and often not at all.

I will remark here, in passing, that physiologists have found a much larger number of white blood corpuscles in the blood of the hepatic veins than in the great portal vein. Their inference, it appears to me, is that these white corpuscles are formed in the liver. I do not say that the liver does not help to perfect them, but they have overlooked the fact that the hepatic veins convey a much smaller quantity of blood to the vena cava than is brought to the liver by the great portal vein, not to speak of the hepatic artery. But this is a digression.

Now let the reader consider well that no physiologist denies that in the pathological conditions referred to a very large

amount of effete matter is carried to the liver. And let the reader also remember that physiologists all teach that effete matter, let it be formed however it may, is excrementitious.

Now, I ask, why is this excrementitious effete matter carried to the liver? Something, we know, is separated from the blood passing through the liver; that is, two pounds and a half of bile in a man weighing one hundred and fifty pounds every twenty-four hours. Physiologists all teach that this excrementitious effete matter passes through the liver untouched; that is, it is not separated from the blood, and that it goes to the kidneys to be eliminated from the blood by them.

They teach, I know, that urea and other oxidized products are formed in the liver, but they do not say that they are formed from effete matter. Indeed they deny it, and maintain that urea and other products are formed from disintegrated albumen and fibrin. They do not say that this disintegrated albumen and fibrin are parts of the effete matter brought to the liver. I teach that all the effete matter brought to the liver is always converted into bile when the liver is able to perform its functions healthfully; and as this bile is formed out of effete matter, it is necessarily excrementitious.

But when the functions of the liver are not healthfully performed, when the effete matter is not all converted into bile, then, and only then, a part of it passes to the vena cava through the hepatic veins, and extra work is thrown upon the kidneys as well as upon the lungs and skin.

There are certain pathological states, of which Asiatic cholera is the fullest expression, in which the functions of the liver are suspended, and the effete matter passes through that gland unchanged. Other pathological states approximate this. In all of these conditions mercury is the efficient remedy. Explain it as you may, it removes the morbid conditions.

This is my explanation of the value of calomel and blue-mass in the treatment of chronic diseases. But I do not confine myself to these two drugs.

Ipecac, aloes, rhubarb, and colocynth are all valuable in the

treatment of the majority of such affections. Besides the saline cathartics, particularly Crab Orchard Salts, often give prompt and permanent relief. I do not exclude other remedies, but if a physician would use any one of them properly, he should know why he should use them, otherwise he will practice empirically.

Now I maintain that I have explained the principles upon which a rational practice is founded. But in this consideration we must not overlook Dr. Cooke's theory of congestion, which occurs in so many chronic maladies.

When the kidneys are at fault, and the states of the urine are morbid, we must bear in mind that the renal veins have no valves, and that the congestion of the vena cava and its branches also directly affects those glands. It must also be borne in mind, that the renal veins enter the vena cava below the mouths of the hepatic veins, and that when the urea formed in the kidneys does not all pass through the ureters into the bladder, then that this urea may, and does, pass through the renal veins into the vena cava.

This is a pathological state, and explains why urea is found under such conditions in the blood of the general circulation. It never occurs in a state of perfect health.

To relieve, then, lithemia, and many forms of dyspepsia which are associated with it, we must see to it that the liver performs its functions healthfully.

Dr. Murchison very correctly says, that this can often be better effected by a proper system of diet, including both food and drinks, than by any other method. Outdoor exercise, often horseback exercise, contributes greatly to this end.

Maladies affecting the brain and nervous system; maladies affecting the urinary and genital organs; and particularly maladies peculiar to women, are all to be treated upon the same principles.

From my very limited knowledge of the use of the microscope and of chemical analysis—in a word, of histology—I do not doubt that they are one and both of great value in making

out a correct diagnosis. But, in my opinion, too much importance is attached to them at this time, because the conclusions arrived at are not sufficiently definite and positive.

From discussions which are published in medical journals it is manifest that these so-called sciences are but yet in their infancy.

The Scribes and Pharisees paid tithes of mint, anise, and cummin, but omitted the weightier matters of the law. We should not follow their example.

CHAPTER XV.

CHOLERA.

The Abbe Huc, in his journey through the Chinese empire, gives the following graphic account of the majestic and awful phenomena which heralded the first appearance of cholera in that country. He says, "We have the account from a great number of the inhabitants of the province of Shan-Tung who were eye-witnesses of what they related. In the first year of the reign of the deceased Emperor—that is to say, in the year 1820—a mass of reddish vapor was noticed one day upon the surface of the Yellow Sea. This singular phenomenon was observed by the Chinese of the province of Shan-Tung, which forms its coast. The vapors were at first light, but gradually increased, became condensed, rose little by little above the surface of the water, and at last formed an immense red cloud, which remained for several hours floating in the air. The Chinese were seized with terror, as they mostly are in the presence of all great natural phenomena, and sought in certain superstitious practices of the bonzes the means of averting the threatened calamity."

"While the inhabitants of Shan-Tung were seeking to conjure away this unknown misfortune, which yet every one foresaw, a violent wind suddenly began to blow, and, dividing the cloud into various columns, drove them on toward the land. These red vapors spread in a winding course along the hills and valleys, and swept over the towns and villages, and wherever they passed men found themselves suddenly attacked by a frightful disease,

which in a moment deranged the entire organization, and changed a living man into a hideous corpse."

I do not know that we know any thing more about the cause of cholera to-day than did the Chinese when the Abbe Huc visited them. He says that it struck on all, rich and poor, young and old, sometimes on one side and sometimes on the other, but always apparently in the most capricious manner without following any fixed rule in the midst of its fearful ravages. It ravaged first the province of Shan-Tung, then turned northward to Peking, striking always in its march the most populous towns. It is not my purpose to give the history of cholera. There may be said to be two theories as to the origin of cholera in different years and in localities widely separated. The one is that it is carried along the great thoroughfares of travel; the other is that it is of local origin, let it appear where it may.

Those who maintain that it is of local origin say that it is produced or caused by malaria, miasm, or the decomposition of animal or vegetable matter, particularly of the latter. Dr. John Esten Cooke and Dr. Theodore S. Bell are among the most distinguished American physicians who believe in the malarial origin of cholera. I can not agree with these distinguished writers. I have referred to the tide-water region of Virginia. It is a sand-stone country with soft spring-water. Miasmatic or malarial fevers prevail in that region with the regularity of the summer and autumnal seasons, and yet, so far as I have been able to inform myself, the cholera has never appeared in that portion of Virginia, except in the larger towns, as Richmond, Petersburg, and Norfolk. I believe the same to be true of Maryland, North and South Carolina, and Florida. If malaria was the only cause of cholera, it certainly ought sometimes to have appeared among the farming community living on the rivers which empty into the Chesapeake Bay. That accumulated filth and stagnant pools of water may aggravate the disease and make it more fatal in localities where cholera prevails, I have no doubt; that the drinking of bad water is often the proximate cause of the attack I have also as little doubt. When I come to explain what I

understand to be the pathology of cholera and the distinguishing features of that malady, I think I will be able to show more conclusively that it is not of miasmatic or malarial origin.

Dr. Charles Caldwell says, "We believe the complaint to be not contagious, but a true epidemic, subject to the laws which govern other diseases of the same class. Like all other epidemics, it travels much more rapidly than any complaint simply contagious can do, its march being sometimes from fifteen to eighteen miles a day. Within the last fourteen years it has overrun a greater extent of the surface of the globe than the smallpox has ever done in a century. It banishes from the place where it prevails all other general diseases, or assimilates them to itself. Neither typhus fever, bilious fever, nor even pestis vera can exist in their common forms within its limits. The reason is obvious. It has possession of the atmosphere, and is therefore supreme. This is another law of genuine epidemics." (*Transylvania Medical Journal* for 1831.)

In my opinion all quarantine laws are utterly powerless to prevent the appearance of cholera in any city, town, or place. The all-important thing to know about cholera is, what is the nature of the disease, or what are the conditions of the malady to be prevented, or to be relieved; and the second equally important thing is to know how to cure it. Let us first endeavor to understand the nature of the disease. Before we undertake to do this let us ask ourselves, Is cholera a curable disease? The professor of the theory and practice of medicine in the Medical Department of the Louisville University, Dr. T. S. Bell, says that it is not. The Abbe Huc informs us that the Chinese entertained the same opinion fifty years ago.

Dr. John Mackintosh treated cholera when it first appeared and prevailed in Edinburgh. He had charge of the Drummond-street Hospital, into which establishment there were received four hundred and sixty-one patients, of which number two hundred and ninety-one died; of these two hundred and eighty were examined most minutely to ascertain the cause of death.

"The morbid appearances peculiar to cholera observed in those

persons who died in the collapsed stage.—The blood attracted our attention in the first dissection, and it had the same appearances to the last. It was dark colored, and had lost much of its fluidity; this was expected, from the accounts that had previously reached us from other countries. But we were astonished to find that it was contained in the arteries and veins, in the most minute capillary, as well as in the larger vessels; that it had the same dark color in both sets of vessels, some of them containing a small quantity, others being enormously distended. The capillaries and large veins on the surface of the body contained as much blood after death as during life. On opening a vein in the dead body, the blood flowed almost as readily as it had done during life in the same person. The surface, therefore, retained the same dark appearance as it presented during life, and the muscles were of a dark-red color. In the act of death, or immediately afterward, in all other diseases, the blood leaves the capillaries, recedes from the surface, and collects in the heart and large veins near it; the arterial system is generally quite empty, but occasionally a little blood is found in the aorta. Here are at once observed three remarkable facts: First, an alteration in the appearance and consistence of the vital fluid; second, a change in its distribution; third, blood can be drawn from a vein almost as readily after death as during life; and the important circumstance may be noticed, that there was an appearance every where of an abundance of blood. Every incision that was made, even in parts not depending, occasioned a flow of blood, so as often to be troublesome by impeding our examinations. Some thought the blood oily.

“In the Head.—Great vascularity was observed on the surface of the brain and in the membranes; not only were the capillaries injected, but the trunks of both arteries and veins were filled with blood—the vertebrals, carotids, and circle of Willis, as well as the vena Galeni, and the longitudinal and lateral sinuses. In the longitudinal and lateral sinuses, however, the blood was not always in a semi-fluid state, but often coagulated; and sometimes there was a fibrinous clot extending through

the course of the sinuses of the brain, into the jugular veins. This appearance of fibrin was observed also in those who died in the consecutive fever. On the lateral surfaces of the hemispheres of the brain we frequently observed an extensive ecchymotic patch; sometimes there were several patches of this kind. This appearance was produced by an effusion of bloody serum between the arachnoid and pia mater. The injection on the surface of the brain was more florid than that in any other part of the body. The ecchymotic spot occupied in some cases only about an inch and one half in length; in others it was very extensive, involving the whole of the hemispheres, and occasionally extending down between them. The vessels of the pia mater, the velum interpositum, the plexus choroides, and the lining membrane of the ventricles, were injected. The surface of the fourth ventricle, in general so white, was seen vascular, occasionally slightly stained with blood. The ventricles, whenever they were examined with a view of ascertaining the point, were found to contain a considerable, sometimes a large, quantity of serum. Sections of the brain displayed the cortical substance much darker than usual, and the brain generally exceedingly vascular. As soon as a section was made there immediately appeared numerous large drops of blood, in size and number much greater than is observed in other diseases, even in active inflammation of the brain. In above one hundred and fifty cases, the spinal marrow and its membranes were minutely examined. In all there was a very considerable quantity of serum, the membranes highly injected, the rachidian veins gorged with dark-colored blood, and the substance of the spinal marrow, in a few cases, appeared a little softer in texture than natural. In a large number of subjects there were ossific depositions, in the form of scales, seen on the arachnoid surface; occasionally they were very numerous and large. The general practice was to place the subject on the face as soon as death occurred, with a view of preventing engorgement of the spinal marrow and brain from a depending position.

"In the Thorax.—The lungs were found gorged with dark,

viscid, oily-looking blood; they were heavier than natural, in some instances weighing three pounds nine ounces. Pleura minutely injected; in those who died rapidly, both the pleura and pericardium had a dry appearance; in other cases the pleura had an unctuous feel, also the serous surface of the pericardium and heart. Ecchymotic spots, of the form and size of *patechiæ*, were frequently seen on the pleura costalis and pulmonalis, extending in many instances a line or two into the substance of the lungs. Occasionally, in those who died in this stage, there were seen one, or perhaps two, small portions of the lung indurated and stained of a dark-red color, presenting all the characteristics of 'pulmonary apoplexy;' this appearance, however, was more frequent in those who died in the consecutive fever. The bronchial membrane was injected, the tubes occasionally gorged with mucus of various degrees of tenacity and tinges of color; the surface of the heart and large vessels very vascular, presenting many ecchymotic spots, more particularly on the acute margin of the right ventricle and the aorta. In many instances these were found to extend deep into the subjacent tissues. On making sections, to display the cavities of the heart, the left ventricle was almost invariably found in the state of hypertrophy, with diminution of the cavity, and generally empty. In the right auricle and ventricle there was found a fibrinous clot, sometimes white, like coagulable lymph, at others stained with blood, consisting partly of lymph and coagulated blood of a dark color. When a mass of lymph was found in the right auricle and ventricle, it invariably extended into the pulmonary artery, and in many cases could be traced into the smallest ramifications; and sometimes the pulmonary veins had a similar plug. On several occasions, the auriculo-ventricular opening was closed by the plug, prolongations from which were found interlacing between the *columnæ carnæ* and *cordæ tendinæ*. In the inner surface of the aorta, and in a few cases in the pulmonary artery also, there was seen a distinct false membrane, completely covering the inner membrane, and extending into the vessels that are given off from it; this membrane

did not always seem to be of recent date, but in many of the dissections it was observed in an incipient state. It was most completely formed near the heart; and on some occasions it was seen below the arch of the aorta, in the act of forming, presenting an appearance like tenacious mucilage, continuous with the portion already organized. Occasionally it was tinged of a dark, sometimes of a bright-red color; but generally it was white, and easily separated from the proper lining membrane, even with the handle of the scalpel. In the few cases in which it was found in the pulmonary artery, it was thinner and not so completely organized. In the aorta, we frequently traced it to the bifurcation of the iliacs; sometimes half way from the heart, perfectly organized, the rest being in a gelatinous state. When separated, the proper shining, smooth character of the inner membrane was seen beyond all doubt, except at parts where there were atheromatous depositions, which were sometimes confined to the false membrane, at others extended into the proper coats of the artery. We carefully removed the contents of the thorax, not only with a view of submitting them to minute examination, but also to investigate into the condition of certain nerves and ganglions.

"I shall now show the state in which we found the pneumogastrics, phrenics, splanchnics, and simular ganglia. The dissection of the neck showed minute injection of the large vessels, both sets containing dark-colored blood, more particularly the veins, which were often not only full but distended.

"*The pneumogastric nerve* was frequently seen stained of a dark-red color through its whole course in the neck and thorax. Sometimes there was merely vascularity on its surface, till it crossed over the subclavian artery, where, in many cases, it was enlarged so as to resemble a ganglion. This enlargement was always of a bright-purple color, and existed on the right side only. But the nerve was frequently similarly tinged at this point through its whole substance, when there was no enlargement. On tracing these nerves onward in their course, they frequently presented a red appearance.

"*The phrenics*, as they passed over the pericardium, were observed to partake of the general injection; and when the pericardium presented a half-dried appearance these nerves were similarly affected.

"In a great many cases we carefully traced the splanchnics on both sides of the spine to the semilunar ganglia. In this part of the thorax there was minute injection of the vessels, ecchymotic specks, like petechiæ. These nerves were implicated in the injection on their surface, but in two subjects only was there any discoloration or other mark of disease in the substance. In one or two cases it was thought the ganglia were somewhat changed from the natural color, but we discovered our error, having had, at that period, several opportunities of examining these ganglia in persons who died of other diseases, when a similar appearance was seen.

"*Abdominal Organs.*—In the stomach, in two or three cases, we found a considerable quantity of undigested food that had been eaten a few hours before the attack, and on one occasion a number of small stones, pieces of slate, and tiles. In some cases there was considerable injection of the peritoneal surface of the viscera, but this was by no means frequent. The stomach was, in general, contracted, sometimes remarkably so, and several times divided by contraction, in the center, into two cavities. The intestines contained more or less of a matter similar to that vomited during life. Unless the patient lingered long in the second stage no appearance of bile was seen in the bowels. The mucous membrane of the stomach was occasionally, but not always, vascular; sometimes quite white, but almost always much softer than natural, and in many cases thickened and quite pulpy, so as to be removed with the slightest touch of the handle of the scalpel. The mucous membrane of the intestines was, in general, more vascular than that of the stomach, sometimes more minutely injected than if size and vermilion had been thrown into the vessels. Occasionally there was ecchymosis, and frequently softening of the mucous membrane, sometimes ulceration, particularly in the ilium and colon. The mucous follicles were gener-

ally enlarged, and Peyer's patches, so rarely seen in adult age, were seldom wanting; they were large, elevated, soft, and spongy, and sometimes slightly ulcerated. In many cases we found the colon, and sometimes the ilium, thickened, the mucous membrane soft, dark colored, and disorganized, as in some of the worst forms of dysentery. The liver was frequently diseased, and the disease not of recent date. Occasionally this organ was very vascular, and we rarely missed seeing sufficient quantity of bile in the pori. In two instances only were there such appearances of engorgement as are described by the India writers. The gall-bladder was, in every instance, filled; sometimes distended with dark-colored and somewhat viscid bile, the organ itself being very vascular, and in many instances containing gall-stones. In no instance, save one, did we discover any impediment in the passage of bile through the ducts into the duodenum. In that solitary instance a spherical-shaped calculus obstructed the passage. The kidneys were generally diseased. The disorganization described by Dr. Bright was very frequently met with. The vessels of these organs were almost uniformly highly injected—a puriform fluid was always found in the papillæ; the bladder was always contracted, so as to be as small and dense as a virgin uterus." (Mackintosh's Practice, vol. 1, pp. 387, 388.)

By reference to a former chapter in this book the reader will find a notice of the discovery of valves in the veins by Fabricius, the master of Harvey, to whom he demonstrated his discovery.

"Harvey observed the knots in the veins of the arm when a ligature is applied, as for phlebotomy, and showed that the spaces between these knots, which are formed by the valves, could be emptied of blood by pressing toward the heart, and would not fill with blood whilst the finger was kept at the lower extremity. It was impossible by pressure with the fingers to force the blood back through one of the valves." This is the condition of the valves in the veins in health.

By reference to the extracts from Dr. Cooke's work, paragraph 1244, chapter 9, the reader will find the following as to the condition of the valves: "It was, when thus laid open to

view, at a single glance apparent that the right side of the heart, the cava, the subclavian veins, the iliac, the crural, and the deep vein running along with the femoral artery down as far as the calf, were very full of black blood. The veins were so full as to preserve the round shape, and were stuffed full to the valves near the head of the humerus, at which point the stuffed appearance ceased suddenly; and immediately at the termination the vein returning from the arm was empty, flaccid, and transparent, so as to scarce be perceived by candle-light. On pressing the blood of the full venous cavity toward the stuffed end at the head of the humerus it would not pass into the vein beyond the valves; but on increasing the pressure it was forced out of the minute veins entering into the subclavian from the skin and other neighboring parts, which had been cut across in laying bare the subclavian vein, and which were so minute as not to let the blood pass till pressure was made."

The reader will observe that in the body of this dissected child the valves and the veins could not be forced to give way even when great pressure was made upon the large and exposed venous trunks. It shows clearly that these valves prevent the blood from returning into the veins, where they exist even after death.

I will call the attention of the reader to the facts observed by Dr. Mackintosh, that the blood was less fluid than usual, and that there was a large quantity of serum in the sinuses of the brain and in the spinal column. He will also observe that there was an unusually large amount of congested lymph in the heart and larger blood-vessels.

I stated in a former chapter that I would have occasion to notice the fact that there are valves in the lymphatic vessels. It would seem, according to the dissections of Dr. Mackintosh, that the lymphatic vessels had all emptied themselves into the thoracic duct and into the right side of the heart. The condition of the blood and of the veins and arteries presented by these dissections show that the valves have entirely lost their vital or natural power. What was the condition of things which forced

them to give way? It was discovered by the first anatomists who made post-mortem examinations that the arteries were empty in every part of the body, while the veins in the three cavities of the skull, chest, and abdomen were found filled with blood. The dissections of cholera subjects made by Dr. Mackintosh were the first, so far as I know, to present an equal fullness of the arteries and veins, and particularly of the external veins which have valves in them. Dr. Mackintosh represents the condition of things which preceded death in these subjects as follows:

He says, "The duration of the premonitory or first stage is various; sometimes the unpleasant symptoms suddenly cease and the patients recover quickly; but this happy issue is comparatively rare when proper remedies are not used; and in some few cases, from the peculiarity of the constitution of the patient, remedies seem to have little effect in arresting the progress of the disease, even when applied in this early stage. The stools, which were at first feculent and bilious, now become characteristic of the true Asiatic cholera. They have the appearance of very thin gruel, or rice-water; sometimes they are watery, limpid, with small flakes of curdy-looking matter intermixed; at other times they present an appearance of water in which fresh beef had been macerated. The usual feculent smell has vanished, instead of which the stools have a peculiar odor, which struck me to resemble that produced by macerating fish in water; a similar odor is generally observed from the surface of the body. More rarely the stools look like the lees of port wine; and it was remarked that almost none recovered who passed "port wine stools." I recollect at present one recovery only in which there was this appearance—it was the case of Field, who was saved by saline injection into the circulation. The desire to go to stool is irresistible and instantaneous; tenesmus is great in some cases, sometimes preceded or accompanied by a sense of heat or griping. The stools are generally very copious—sometimes, however, scanty; often accompanied by loud discharges of flatus from the bowels. Along with the bowel complaint there is burning heat in the region of the stomach, and vomiting of large quantities of

a similar fluid from the stomach. The abdomen feels doughy. The thirst is intense, and there exists an urgent desire to drink cold water. The mind, for the most part, remains comparatively entire, but the vertigo and tinnitus increase. Cramps are general attendants—sometimes confined to the fingers and toes; at other times they affect the muscles of the extremities, and often those of the trunk of the body, more particularly of the abdomen. The urine is generally suppressed early in the disease. The voice is whispering, the person being unable to speak in any other tone. The respiration, although weak, is often nearly natural in other respects, even at times when the pulse is scarcely perceptible at the wrist; occasionally, however, the breathing is hurried and oppressed, sometimes laborious. The pulse becomes weak and rapid early in the disease, even when the action of the heart is comparatively strong and tumultuous; but frequently both the pulse and action of the heart are feeble. As the disease goes on both become more and more weak; the pulse is only now and then felt like a “flutter,” and often ceases to be perceptible at the wrist for some hours before death. The tongue is cold and shrunk.

It is quite painful to a bystander to watch the restlessness and impatience of the sufferers, who are constantly in a state of jactitation, more particularly when restrained and when heat is applied. Indeed they seem to have a horror of, and to suffer pain from, warm applications. The temperature of the body, but more particularly of the extremities, diminishes early in the disease, and goes on sinking. It is often impossible to raise the temperature of the body during life, but the moment death takes place, and for two or three hours afterward, the body becomes warm—even the icy coldness of the extremities gives place to a genial warmth. The color of the hands and feet becomes changed, more particularly the nails assume a blue appearance; the face often is similarly affected; occasionally the whole surface presents a blue color, and, consequently, the second stage has sometimes been termed “the blue stage;” but it is an error to suppose that the blueness is invariable, or that it

is an attendant only on the worst forms of the complaint; the patient who had this appearance more strongly marked than any other was the one who made the most rapid and the most complete recovery. Blood drawn from an artery or vein during this stage flows with difficulty, is of a dark color, does not coagulate or separate any serum. It remains in a semi-fluid state, and has the appearance which the ancients called "dissolved blood." The surface of the body is covered, for the most part, with a cold exudation, the features and eyeballs shrink, and death closes the scene—sometimes very unexpectedly, at others the body seems to have been long dead, while the functions of brain are still going on and are comparatively entire." (Mackintosh's Practice, vol. 1, pages 378, 379.)

Considering now only the cases which died, it will be seen that in them the conversion of food into blood very soon ceased. In cholera the first symptom which attracts attention is the copious pouring out of watery discharges from the bowels. Very soon after these discharges commence, the vital processes for the conversion of food into blood cease to act.

The poison which produces cholera, whatever it may be, differs from miasmatic or malarial poison in this, the first causes the serum of the blood to be poured out through the mucous membrane of the stomach and bowels, producing copious vomiting and profuse diarrhea, while the latter (miasmatic poison) ordinarily does neither. The diarrhea commonly precedes vomiting, and it sometimes continues for some hours before the strength of the heart's action is much disturbed. The ague of an intermittent fever, and the cold stage of congestive fevers are sometimes attended by as much coldness of the limbs and body, felt by the patient, as the cold or collapsed stage of cholera. The difference is in the sensation of cold. In the collapse of cholera the breath is cold and the temperature upon the surface, measured by the thermometer, is no doubt lower than the temperature of the surface in an ague; but in the latter, if the patient is dressed and sitting up, he wants to get near the fire, if in bed, he asks for more blankets. In the collapse of cholera,

on the other hand, the patient does not complain of cold, and it is unpleasant to him to be near to a fire, and when in bed they very commonly ask that the bed-clothes be removed; and yet patients constantly recover, in all parts of the world, from the ague of an intermittent. I have been familiar with intermittent fevers since my childhood, and never knew or heard of a patient dying in the cold stage of an intermittent fever. Patients also constantly recover from the first and second paroxysm, or cold stage of congestive chills. So well marked are the stages of these forms of miasmatic disease, that periodicity is considered as one of their remarkable peculiarities. Ordinarily reaction always follows the ague or cold stage, which is followed in its turn by the sweating stage. But in these miasmatic maladies there is no pouring out of the serum of the blood by excessive diarrhea. This discharge of serum alters at once the healthy condition of the blood, and the greater the loss of serum the more complete is the altered condition of the blood. As remarked by Dr. Mackintosh, it becomes thicker. This loss of serum and the thickened condition of the blood immediately disturb the action of the heart and the entire circulatory system. Minute by minute, as the action of the heart in cholera becomes weakened, less blood consequently passes through that organ and through the lungs. This necessitates the accumulation of blood in the vena cava and the large veins emptying into it. Dr. Mackintosh found the vena cava distended to one and three fifths of an inch in diameter. Ordinarily its diameter is one inch.

Up to a certain time in the progress of these pathological conditions there is a greater accumulation of blood in the veins, which make up the portal system, than there is in the veins found in the cavity of the chest or of the skull. The first alvine discharges, which constitute the diarrhea of cholera, are mixed with the fecal matter and bile then found in the bowels. But very soon the fecal matter and bile are no longer to be seen. The liver has ceased to secrete bile. The only form of bile which can be secreted under this condition of things is that

formed from effete matter. This is accumulating rapidly. But the blood has become altered, thickened, by the pouring out of its serum. This applies to the arterial as well as to the venous blood. The action of the heart is already weakened, causing less blood to be sent through the arteries. Very soon after the peculiar symptoms of cholera begin to intensify, the breath becomes cool, and the blood, receiving less and less oxygen as it passes through the lungs, is not arterialized. Consequently a smaller quantity of blood, not properly oxygenated, is sent to the liver through the hepatic artery. The liver, like every other gland, depends upon the arterial blood thus sent to sustain its vitality, and to enable the secretory organs found within it to perform their functions. We have here in the liver a double difficulty. Thickened blood, not normal in any particular, is brought to it by the portal vein; and the arterial blood brought to it through the hepatic artery is deficient in vital elements. Unless the disease is arrested these difficulties are continually increasing. They are sufficient at the beginning of the disturbances, already alluded to, to arrest the secretion of bile in the liver. Indeed, every secretion in the body may be said to be arrested; for the pouring out of the serum of the blood under the form of rice-water discharges, and the cold sweats upon the surface of the body, can not be called secretions at all. I maintain that bile is secreted from effete matter, as well as from food, and say that, in the condition of things referred to, it can only be formed from effete matter floating in the blood. Physiologists teach that it is formed or secreted from the blood—how, they do not explain; but they certainly teach that it is continually formed and continually poured out, in the duodenum, in the state of health at least.

It matters not whether my explanation as to the source and formation of bile, or their *no* explanation, be correct in this inquiry. Be that as it may, if the bile is not secreted by the liver from the fluid brought to it by the portal vein, another disturbing cause must be at once set at work. It is the province of the liver to remove two pounds and one half of bile from the blood

every twenty-four hours. So say all the books. Somehow or other, or from some cause or other, this bile is not removed. Then its constituent elements must remain in the blood, and this at a time when the vital fluid is becoming thicker every moment by the pouring out of its fluid parts through the bowels and the skin, and becoming darker or blacker by not receiving the healthful quantity of oxygen as it passes through the lungs. During this state of things the blood passes very slowly through the liver; it becomes congested in that organ.

I say the blood passes slowly through the liver; it simply passes through, because it is not acted upon by the secernent organs. All of this increases the congestion of the portal veins, of the pancreas and spleen, and of the great veins found within the abdomen. The spleen has ceased to empty rejuvenated blood containing an increased quantity of albumen and fibrin into the veins which convey the blood from it. The secretion of urine by the kidneys begins to be suspended at a very early stage in the progress of the pathological conditions which constitute cholera; and it soon ceases altogether. If the kidneys act upon effete matter, it would seem that now was the time, of all others, that they should be actively at work. But the conversion of food into blood has stopped entirely, and they have no longer any thing to do in the blood-making process. They do not even separate water from the blood. The skin and mucous membrane of the bowels have this pathological work thrown upon them. But the kidneys are very soon rendered incapable of performing any secretory function. There being no valves in the veins which convey the blood from the kidneys to the vena cava, the renal veins are very soon filled with blood as the consequence of the distended fullness of that great vein. This congestion of the kidneys places them in a pathological condition. A time soon arrives when there may be said to be a separation between the brain and nervous system and the rest of the body. Those who have used the galvanic battery with the copper trough and zinc plate suspended in it, know that the battery and the trough are connected by two short, bowed, copper wires. If either of

these wires is removed when the battery is actively at work, it stops instantly. I can not better illustrate what appears to me to be the condition of things in the body of a cholera patient, when the operations of the mind are clear and intellectual while the body is seemingly dead, than by the above comparison. But there is another way of illustrating it. It is well known to all who are accustomed to use the kind of battery spoken of, that the solution of sulphate of copper (the fluid commonly used), must be of a certain strength; otherwise the battery will not work, though the connecting wires be in place, and every thing else all right. Now in the human body the blood represents the solution of the sulphate of copper which fills the trough in which the zinc plate is suspended. The blood in a cholera patient has become so altered by the various morbid processes to which it is subjected, as stated above, that it is no longer able to stimulate, to excite, or to put into active operation the brain and nervous system, which may be called the galvanic battery of the animal body. It is when this break between the batteries of animal and organic life occurs that the valves in the veins become powerless. Up to a certain time in the cold stage of cholera the surface of the body has a shrunken appearance, all the smaller veins having apparently emptied themselves. The action of the heart at this time being very feeble, very little blood is sent into the arteries which go to the surface of the body. So long as the valves in the veins continue to have their natural power, the blood continues to be emptied by these veins into the larger veins within the chest and abdomen, so as to increase the accumulation of blood in the great veins found in these cavities. But when the valves give way the whole system of blood-vessels, arterial as well as venous, including the heart, seem to become as so many caoutchouc tubes and pouches, and the blood is forced through them all indiscriminately as through a sponge. I suppose that the pressure of the atmosphere upon the human body would be sufficient to do this. Dr. Mackintosh states that the lungs are found excessively congested; in one instance they weighed as much as three pounds and nine ounces.

I have already alluded to the fact, which must be admitted by every one who has any knowledge on the subject, that when the action of the heart becomes exceedingly weak, as is always the case in the collapse of cholera, but a very small quantity of blood is sent to the lungs through the pulmonary artery. The congestion of the lungs, then, can not be caused by the amount of blood brought to them through that artery, for as long as the patient continues to breathe and the heart to beat, the blood carried to the lungs through the pulmonary artery will return to the left side of the heart through the pulmonary veins. This congestion of the lungs, then, approaching almost to pulmonary apoplexy, must be accounted for in another way.

In another chapter, where I have spoken of the azygos vein, I stated that the bronchial veins emptied into the azygos vein, and that these veins have valves in them. I suppose that the excessive congestion of the lungs found by Dr. Mackintosh in cholera subjects can only be accounted for upon the supposition that these valves had given way; as they had also done in the superficial veins. When these valves do give way, the enormously distended veins within the cavities will then relieve themselves, in part, of their excessive congestion. The bronchial veins then become filled with the blood which is forced back into them from the vena cava. The blood within the cavities being still warm, when it thus returns to the external parts of the body, produces a corresponding warmth in them. But it is in this way that I account for the bodies of cholera subjects becoming warm after death, when during life they were icy cold.

I saw a striking instance of this in 1849. The patient was a middle-aged woman. I saw her a short time before she died, when her limbs and body were unusually cold even for a cholera patient. Several hours after her death, when they were preparing to bury her, I was sent for. Her body had become so warm that the attendants thought she was coming to life again. This state of things does not occur in all cases; but when it does occur, my explanation is, I think, the only intelligible one. It

is only a supposition, but I conjecture it, that the valves begin to give way before death.

I have thus briefly given my views as to the pathological conditions which occur in cholera patients who die. They are based upon the post-mortem examinations made by Dr. Mackintosh. I have treated patients during three epidemics of cholera—1849, 1853, and 1855. No post-mortem examinations were made here. The symptoms and the stages of the disease corresponded exactly with those reported by Dr. Mackintosh as occurring in Edinburgh. The post-mortem appearances observed at different times and at different localities, by physicians of eminence who made them, differ in some respects a good deal from those observed by Dr. Mackintosh at Edinburgh. This difference applies particularly to the condition in which the lungs, the brain, and the spinal cord were found by him.

Dr. Austin Flint says, in his *Practice*, "The lungs, in some cases, contain but little blood, and in other cases more or less hypostatic congestion is observed." He says, "The spinal cord offers nothing abnormal. The ganglions and nerves of the sympathetic system present nothing abnormal." The same conditions have been observed by many physicians both in this country and in Europe. Indeed, so far as my reading goes, the lungs have been found comparatively empty of blood. All physicians who have treated cholera agree, I think, in this, that during the cold stage very little blood passes into the lungs through the pulmonary artery; so that it would be unreasonable to expect to find them filled with blood, as observed by Dr. Mackintosh, unless the blood is forced into them through the bronchial veins, whose valves have lost their natural power, as I have already explained.

In Dr. John Chapman's work on *Diarrhea and Cholera*, in a note at page 246, you find the following: "Post-mortem state of the sympathetic system and the pneumogastric nerves in cases of death during the choleraic collapse. . . . He, Dr. George Steele, says, 'I am anxious to call attention to one feature which was very constant in the cases I examined, and which, though it

can not be without importance, has, apparently, either not existed or escaped the notice of writers since 1832. I allude to a singularly hyperemic state of the sympathetic system and the pneumogastric nerves. It was not unusual to find congestion more or less extensive in every portion of the sympathetic chain of ganglia. The splanchnic nerves, the semilunar ganglia, and the solar plexus were the parts chiefly involved, and these not unfrequently were imbedded in extravasated blood.

“The celiac, renal, esophageal, cardiac, and pulmonic plexuses exhibited morbid lesions of the same kind, but generally to a less extent. The neurilemma of the pneumogastric nerve frequently injected, and studded with ecchymosed spots; and in one case I found the phrenic nerve, where it spreads out upon the diaphragm, steeped in a large coagulum. In one case also Mr. Lizars found the same nerve involved, its neurilemma in the thorax being injected all the way to the diaphragm.”

The reader will observe that Dr. Steele says that the post-mortem appearances observed by him had either not existed or escaped the notice of writers since 1832. This was about the time of Dr. Mackintosh's observations. Considering the care with which post-mortem examinations have been made since 1832, it is not reasonable to suppose that any such marked characteristics of the nervous system (post mortem) would have escaped the notice of medical examiners. This is the more particularly true, because since then especial attention has been paid to the nervous system, and no opportunity for examination has been permitted to pass unnoticed. The only conclusion, then, at which any reasonable man can come to is, that the post-mortem appearances in the nervous system observed by Dr. Mackintosh and Dr. Steele are among the unusual conditions observed in subjects who die of cholera.

If the reader will refer again to the chapter in which I speak particularly of the azygos vein, he will find that veins going from the spinal column and the ganglions of the sympathetic nerve empty, for the most part, into the azygos veins. These veins, as already repeatedly said, have valves in them, and

empty themselves into the vena cava, when the action of the heart becomes weakened, as is always the case in the collapse of cholera; and in the approaching collapse very little blood is sent to any part of the body, and all the veins which have valves empty themselves and can not become congested. Dr. Chapman uses the words hyperemia and congestion as synonymous. I have already pointed out the confusion to which this leads. There can not be arterial fullness or hyperemia in any part to which but little blood is sent. There can not then be arterial hyperemia in the spinal column or the ganglions of the sympathetic nerves during the cold stage of cholera, or during the ague of a miasmatic fever. Nor can there be venous congestion in parts where the veins have valves; unless the valves have lost their power, and permit the blood to be forced back into them from the great and exceedingly congested veins within the cavities of the chest and abdomen.

In the subjects examined by Dr. Mackintosh and Dr. Steele I suppose the congestion of the vena cava and of the veins emptying into it was excessive, and that the valves in the bronchial and azygos veins were made to give way; and hence the excessive congestion of the lungs observed by Dr. Mackintosh, and of the spinal column and nerves observed both by him and by Dr. Steele. When the valves referred to give way, death is inevitable. In fact it is the beginning of death. Indeed, after death from any other malady, the valves in the veins still have the power of preventing the blood flowing back into those veins.

All post-mortem examinations of cholera subjects, and particularly those made by Dr. Mackintosh and by Dr. Geo. Steele, show clearly that cholera is a disease in which congestion of the great veins within the cavities is most complete. This congestion, in and of itself, so interrupts the action of the heart as to render the circulation of the blood to be very slow and incomplete. There is then very little motion in the current of the circulation. All glandular secretion is entirely suspended, and hence there is a very slow circulation of blood through the glands. The blood may be said only to pass through them, for

no glandular or catalytic action is exerted upon it. The transformation of food into blood, which according to my theory is going on in every part of the body during health, is entirely suspended. Not only all the actions connected with the vascular system, including the arterial, venous, and capillary, are interrupted; but the lymphatics also, which according to my views, perform an important part in the conversion of food into blood, cease entirely to do the work allotted to them in the animal economy. The lymphatics soon empty themselves of all the lymph which they had formed, and, having valves, they afterward remain empty.

This is one of the explanations why the veins were found by Dr. Mackintosh, and by some other observers, to be so enormously distended; so completely filled, indeed, that they could not hold all of the fluid, called blood, so that even the arteries (post mortem) were found to contain a large quantity of blood.

In the majority of cholera subjects, the liver was found to be congested; but in some it was not particularly so. The reason for this is, I think, of easy explanation. A full, or a larger quantity of blood, if I may so express it, goes to every gland or secreting organ, when the functions of that organ or gland are to be actively performed. The liver, according to my teaching, performs two glandular functions: the one, in acting upon the food in passing through it, and the other in acting upon effete matter. Under this condition of the circulation of the blood the liver might or might not be congested.

As already stated, the conversion of food into blood very soon ceases in a cholera patient; and soon after this the liver is incapacitated to make bile out of effete matter; so that it performs no function at all. In a short time after the rice-water discharges begin to be poured out; in a short time after the action of the heart becomes weakened; in a short time after the whole volume of blood, considered as a whole, receives but a small quantity of oxygen from the air inspired; in a short time after animal heat ceases to be generated, because of impeded

motion in all parts of the body; in a short time after, I say, all these four coincident circumstances, but a very small quantity of blood is sent to the liver through the hepatic artery. Active glandular and catalytic actions in an organ, whose special functions are to secrete, attract blood to that organ; but as no such functions are performed by the liver under the conditions stated, and as the veins which go to form the portal vein are susceptible of great distension, we can readily understand that the accumulated blood within those veins might remain almost stagnant within them, while but a small quantity of it simply passed through the liver. I will not here pretend to say what the cause of cholera is.

This malady existed in India perhaps from time immemorial. It may have existed in China for as long a time. European writers are agreed that it developed itself in an unusual manner in India in the year 1817. According to the Abbe Hue it made a development equally alarming in China in 1819 or 1820. It first appeared in Europe and the United States in 1832. According to M. Hue it was heralded in China by unusual and terrible atmospheric phenomena. In Europe and the United States it has, with a few exceptions, prevailed during the summer months. Its greatest mortality has been when the temperature of the atmosphere was high. In Lexington and, in Versailles, twelve miles from here, persons of the first intelligence, and in every other respect entirely reliable, testify to the fact that, when approaching these towns, they smelt a peculiar odor. Individuals who would leave these places and be absent for a day or more, testify that the olfactories informed them of the fact that they were entering a cholera atmosphere when they returned. Many physicians testify to the fact that, when called to a house, they could tell as soon as they entered, by the smell, whether their patient had the cholera. I do not doubt that the cholera poison is atmospheric; but inasmuch as only a limited number of the population of a town or village is attacked, there must be some other cause or influence why some have the malady while others escape.

Dr. Mackintosh found in his post-mortem examinations appearances which showed the existence of chronic maladies in those who died of cholera. But all physicians know that many young and apparently healthy men and women take the disease and die with it. But this applies to so many other forms of mortal maladies that it is not remarkable. While I believe the disease to be atmospheric, I have no doubt that this atmospheric influence is local; and that it may begin and terminate in this circumscribed locality. If any one will call his attention to it he will be astonished to find how circumscribed and local rains sometimes are. This particularly happens in very dry seasons. During the summer of 1872 there were frequent and abundant rains in Lexington, when on several of the thirteen turnpikes going out of this city there was not rain enough to lay the dust within a mile of the town. In 1833 the cholera was very bad in Lexington, and there was not a case in Versailles, twelve miles off. In 1834 it was terribly fatal in Versailles, while there was not a case in Lexington. In 1849 it prevailed at the lunatic asylum near the city limits for six weeks before there was a case in Lexington, while there was constant intercourse between the two places.

Epidemic or Asiatic cholera is considered to be a different disease from the malady called cholera-morbus, and from the maladies called summer complaint and cholera-infantum of young children. While I do not doubt that there is sufficient reason for these distinctions, I also believe that they both, or all, consist essentially in the partial or total suspension of the conversion of food into blood. When I was a boy, studying natural philosophy, there was a little contrivance used to show the attraction of gravitation and the power of increased velocity. A rope was placed over a beam, at the end of which were two rollers. At each end of the rope a little contrivance was fixed of equal weights; so that they balanced each other. Then a weight which projected beyond this contrivance was placed on one of them. This caused it to descend rapidly; but after falling a few feet it passed through a ring which took off the weight, but yet it continued to descend to the ground. Now I suppose that after the cause of cholera

has so far affected the animal body as to cause the pouring out of the rice-water discharges; so far as to produce a weakened action of the heart with all of the other pathological conditions before referred to, as to the lungs, liver, and glandular system, generally; I say, that if after all these pathological conditions have set in the cause could then be removed, the patient would still succumb unless something was done to arrest these pathological disturbances. There are some persons, and among them may be found physicians, who deny that a patient ever recovered from the malady generally known as Asiatic cholera. I will refer to them hereafter. The medical world, however, both in Europe and in this country, have been agreeably surprised, I may say, at the remarkably quick and complete recovery of many persons who were supposed to be almost *in articulo mortis*. This has been observed in cases too numerous to mention. It goes to show, I think, two things conclusively: First, that the *materies morbi*, or the cause of the disease, is not a poison like unto that of smallpox, of typhoid fever, of measles, and scarlet fever; and second, that up to a certain time in the process of the pathological changes constituting cholera, no organic lesion has taken place which is in itself mortal. During the past year cholera prevailed in Kentucky, in certain towns, with considerable fatality. According to the reports published fully one half of the cases died.

I believe it is now generally agreed both here and elsewhere, in Europe and the United States, that one half of the cholera patients die. It is a very large mortality. It is only necessary to refer to the books written on cholera, and to the medical journals and medical reports, to satisfy one's self as to two facts: First, that there is no agreement as to the essential nature of the disease; and second, that there is less agreement as to the treatment of the malady. The first of these questions I have considered to some extent. The second I will now consider fully, which involves, incidentally, a further consideration of what constitutes the malady called cholera. Among the recent theories on cholera which have claimed the attention of the medical public, I will notice that of Dr. John Chapman, of London.

In his work on Diarrhea and Cholera, p. 99, he expresses his theory in the following propositions: "1. All the phenomena of cholera are due to simultaneous hyperemia of the spinal cord and the sympathetic nervous system. 2. All the phenomena of cholera are naturally divisible into two classes, accordingly as they are referable to the spinal cord, or to the sympathetic ganglia as their cause. 3. All active or positive phenomena are due to hyperemia of the spinal cord. 4. All passive or negative phenomena are due to hyperemia of the sympathetic ganglia."

I think it must be evident to all who have carefully read my book so far, and who have not allowed prejudice or the weight of high-sounding authorities to prevent them from considering what I have written, that this theory of Dr. Chapman is entirely baseless. There has been much difference of opinion as to a correct diagnosis between common diarrhea and epidemic or Asiatic cholera; but no medical man can deny that when the malady has reached a point at which the diagnosis may be said to be clear and unmistakable, there is no symptom more marked and invariable than a weakened action of the heart, and an interrupted circulation. If by hyperemia of the spinal cord and the ganglions of the sympathetic system Dr. Chapman means arterial fullness, I say that it does not exist, and can not exist. When local inflammation is set up in a part there may be a question as to whether it consists in a fullness of the arteries, of the capillaries, or of the veins in the part inflamed. I say medical men may, and I know that they have, differed as to these points. But when there is no inflammation or well-marked irritation even in a part, as there certainly is not in either the spinal cord or the ganglions of the sympathetic system of a patient who unquestionably has cholera, there can not be hyperemia of the arteries going to these parts when the action of the heart is weakened, and is growing weaker every hour. The spinal cord and the ganglions of the sympathetic system not being supplied with the usual and healthful quantity of blood commonly sent to them, can not stimulate the nerves going from them, or either of them, to excessive or unusual action. If by hyperemia of the organs

referred to he means venous congestion, such as was observed post-mortem by Dr. Mackintosh and others, I say, first, that venous congestion never excites or stimulates any organ to healthful secretion; and secondly, that such venous congestion in the spinal cord, in the ganglions of the sympathetic system, and in or about the nerves going off from either of them, only shows a pathological condition which incapacitates one or all of them for healthy action of any sort. The first principles of Dr. Chapman's theory being without foundation in either physiological or pathological anatomy, the superstructure intended to be raised upon it must fall to the ground.

I might here stop in the consideration of his theory, but I will notice briefly several points which he makes; he speaks of excessive activity of the mucous membranes, and of all the glands of the alimentary canal. I deny that either of these conditions obtains in cholera. Perspiration is a natural, healthy function of the skin; but the colliquative sweats of consumption, and the cold, clammy sweats of cholera, can not be said to depend upon the excessive or even active perspiratory functions of the skin; but are rather evidences of the powerless condition of the perspiratory glands to prevent the pouring out of the watery parts of the blood. Healthy perspiration is warm. The sweats of consumptive patients and of cholera patients are cold. The healthful action of the mucous membrane of the alimentary canal is to secrete a sufficient quantity of mucus to lubricate it; but when the quantity of mucus poured out is excessive, it is to be attributed to a deficient action, and is like unto the cold sweats above referred to. Dr. Chapman not only speaks of an excessive action of all the glands of the alimentary canal, but he also refers to the fact that these glands are sometimes found enlarged after death in cholera subjects.

I have in a former chapter referred to the fact, which I think no physiologists will deny, that all healthful glandular action ceases whenever the force and power of the heart's action are greatly increased, as in fever; or whenever, from whatever cause, this force and power are greatly weakened; as all phy-

sicians know that they are in cholera. There is, I affirm, no glandular action of any sort going on in the body of a cholera patient after the attack may be said to be established. The quantity of mucus found in the rice-water discharges is small compared with the quantity of serum, or the watery parts of the blood. They are both poured out because of the excessive congestion of the abdominal viscera, particularly of the stomach and bowels, and because of the want of power in the organs which make up the alimentary canal, glands, and membrane, to resist or prevent the pouring out of these excessive quantities of this so-called rice-water from the blood-vessels. The quantity of desquamated epithelium found in the rice-water discharges is the result of the destructive changes taking place in the mucous membrane of the bowels. The enlarged condition of the glands, found in the bowels after death in many cholera subjects, can have, I think, but one explanation. The enlargement is due to the excessive congestion of the abdominal viscera, and of the bowels in particular. The action of the liver having ceased for some time before death, the veins which go to make up the portal vein, and particularly those which go from the mucous membrane of the bowels, can not empty themselves neither before or after death. Hence these small glandular bodies remain in an enlarged or swollen condition.

At page 106 Dr. Chapman says, "*Abnormally high temperature within the rectum.*—The doctrine that the white substance expelled from the bowels, or found in them after death, is not the result of passing exudation, but is chiefly a product of excessive glandular activity, is in perfect accordance with, and for the first time explains, the fact that during collapse the temperature within the rectum is extraordinarily high, and that it falls simultaneously with the emergence of the patient from that state. In a case of cholera, mentioned in the *Lancet* in 1832, the temperature in the rectum was 105°."

"Late experiments in Paris," as remarked by Dr. Mackpherson, "show that the temperature rises to about 103° in the rectum."

In a case of collapse, "not in a severe form," under the care of Dr. Weber, the temperature in the axilla on the second day of collapse was 97° F., while that in the rectum was 100.5°; on the following day it was in the axilla 97.6°, in the rectum 99.5°; and, again, a day later, in the axilla 98°, in the rectum 99.1°. Dr. Weber remarked that he had also found, in several other instances in which he had examined for it, that during the collapse the temperature in the rectum was more or less above the average of health; that it had been in one case of severe collapse as high as 103°, while in the axilla at the same time it scarcely exceeded 95°; that with progressing recovery the temperature in the rectum had decreased, while that in the axilla had increased. "In interesting accordance with these facts the temperature in the groin is found to be higher than it is in the axilla."

In some remarks introductory to the lengthy extracts taken from Dr. John Esten Cooke's book, I said that I believed that he was the first medical man who fully and correctly understood what was meant by, and what really is, venous congestion. No symptom has been more noticeable in cholera than this, viz. that cholera patients complain always of great internal heat within the abdomen; and crave cold water to relieve, as they suppose, this internal heat; that is to say, they seem to think or feel that a large quantity of cold water will relieve them of their torment.

By reference to the extracts from Dr. Cooke's book, it will be seen that when there is excessive congestion of the vena cava, this congestion extends to the hemorrhoidal veins and to all the veins within the pelvic cavity, including the iliac veins. In the collapse of cholera the congestion of these veins is excessive. The heat in the abdomen complained of by cholera patients, and the heat found by the thermometer within the rectum, is to be accounted for by the excessive quantity of blood which has accumulated within those veins; which blood is undergoing pathological changes. Besides this there is excessive pathological action or motion in the mucous membrane of the stomach and

bowels coincident with the excessive pouring out of the rice-water discharges.

I have said, in another chapter, that heat and motion were coexisting facts. The heat referred to by Dr. Chapman is, then, to be accounted for by the enormously increased quantity of blood found within the abdominal and pelvic cavities, which blood seems to have left almost all other parts of the body, and by the pathological activity involved in the pouring out of the serum of the blood. But there is no glandular action in the case at all. We know that there is greatly increased heat in many fevers correlative with increased motion in the circulation of the blood, but no glandular secretion of any kind goes on while this fever exists. There being, then, an enormous quantity of blood in the abdominal and pelvic cavities, it would of itself account for the increased heat felt in those parts; but when we add to this the excessive action involved in the pouring out or effusion of the rice-water discharges, when they occur, there is no difficulty in accounting for the increased heat complained of by patients and that found in the rectum by the thermometer.

On the cold breath in cholera Dr. Chapman says, "The amount of oxidation throughout the body, the lungs themselves included, being very slight, the temperature of the small amount of blood passing through the lungs is extremely low; and, of course, the very small quantity of carbonic acid and watery vapor which is exhaled being comparatively cool, is able to raise the temperature of the residual air in the lungs, and of that which is expired very slightly indeed."

Dr. Chapman explains this condition by his theory; but what he attributes to contraction of the arteries I explain by weakened action of the heart and excessive congestion of the great veins. The arteries always contract or dilate to accommodate themselves to the quantity of blood passing through them. In the cold stage of cholera, when the action of the heart is excessively weak, but a small quantity of blood passes through the lungs and enters the arterial system. Of course under this state of case the arteries contract so as to accommodate themselves to

the lessened quantity of blood in them. The limited extent to which oxidation takes place in the lungs because of the small quantity of air inspired, and the limited extent to which it takes place in all other parts of the body because of the total extinction of all glandular action, which action involves motion, has very much to do with the cold stage of the patient. That the nervous system is involved there can be no doubt; but there is no lesion of the nerves, except when all chances for recovery have passed away; much less is there any condition which could be properly called hyperemia of the nervous centers.

I will call attention to what Dr. Chapman says in regard to the condition of the spleen in cases of cholera. "In intermittent fever the spleen, as is well known, becomes enlarged, and extremely so if the disease attacks the patient many times. In fact, as a consequence of this disease, rupture of the spleen sometimes occurs. The first distension of the organ is, I believe, coincident with the first onset of the cold stage; and the oftener this stage recurs, the larger the spleen becomes. Now the remarkable way in which the spleen is thus affected during the course of ague, is, in my opinion, an instructive illustration of what occurs in the same organ during the collapse of cholera, as a result of the energetic action of the negative motor nerves. By constringing the systemic arteries throughout the body, and thus emptying, or nearly emptying, all but the larger trunks, they force the blood in every possible direction in which it encounters the least resistance; the spleen is thus made the receptacle of an enormously large quantity, which distends it extremely, and produces the great pain, as well as the sense of burning heat, complained of in the left side. It seems to me probable that the considerable heat of this mass close to the stomach, which is itself the seat of intense glandular activity, and therefore hotter than natural, affords the explanation why cold drinks and ice are so peculiarly grateful to patients in collapse. The post-mortem condition of the spleen of patients dying in collapse, differs, as already stated, in different patients. By referring to paragraph fourteen of the previous chapter, the

reader will, however, find evidence that the spleen is often observed after death to be enlarged, and to present internally vestiges of extreme congestion. Though it is also often found small, this fact does not disprove the hypothesis that it was congested and distended before death; for considerations hereafter adduced justify the belief that in those cases in which the spleen is found of normal size, there has been a reflux of blood from it at the time of and after death produced, as will be described in the paragraph in which I shall account for the elevation of temperature after death.

Referring to a report of seventy cases examined by Virchow, Dr. Gull says, "During life he (Virchow) had repeatedly, by percussion, been able to determine that it (the spleen) was decidedly enlarged; and in some cases, patients had complained of pain in the left side. In old persons no enlargement was ever observed; but in almost all other cases the spleen was so flaccid and shriveled, that one was obliged to admit a previous enlargement, or rather so recent and rapid a reduction of volume that the capsules had not yet accommodated themselves. The change of color in the tissue, from altered blood, led him to infer a previous hyperemia."

Dr. Chapman is certainly correct in supposing an exact similarity in the condition of a patient in the cold stage of an intermittent and in the collapse of cholera, so far as the congestion of the spleen and the veins which go to make up the portal circle are concerned. He says that both are to be accounted for by the energetic action of the negative motor nerves. Were we not familiar with the bewilderments in which a man sometimes finds himself when attempting to explain an absurd theory, it would be difficult to tell how a medical writer of Dr. Chapman's erudition could suppose that there was an energetic action of any part of the nervous system during the cold stage of an intermittent, or during the collapse of cholera. But he is entirely correct as to the fact that the spleen is excessively congested in both of the conditions referred to; the only difference being that the spleen becomes enlarged from congestion after

many repeated agues, while in cholera excessive congestion is coincident with the collapse, which may be called in this connection as one or a single paroxysm of the cold stage.

As before remarked, in pointing out the differences between cholera and intermittent fevers, I said that in the latter there was no diarrhea, while an excessive diarrhea, or the pouring out of the rice-water discharges, was almost a pathognomonic symptom of cholera. This excessive pathological action, not glandular action, but simply action, causes the blood to flow more freely to the abdominal viscera so long as the heart has any power to compel the blood to flow in any direction.

In the condition of things under consideration I would call the attention of the reader to the difference then existing between the liver and spleen; which is this, that but a small quantity of blood is carried to the liver through the hepatic artery, while the over-distended portal vein is pressing upon the liver to allow the blood to pass through it. If the blood could so pass, as in health, the congestion would soon be removed; but inasmuch as all glandular action is suspended in the liver, no bile being formed, the liver can only receive a limited quantity of blood, sufficient, however, to congest its blood-vessels to a considerable degree. But inasmuch as a much larger quantity of arterial blood is carried under any and all circumstances to the spleen than to the liver, the spleen would soon become congested from the supply of arterial blood alone, because the veins going from it are not able to carry off the blood because the portal vein can not freely empty itself through the liver. There is another reason for the excessive congestion of the spleen in the condition of things under consideration, to wit, there being no valves in the veins coming from the spleen, and those veins being large and comparatively short, the large amount of blood coming from all other parts of the abdominal viscera is dammed back into the splenic veins.

I agree with Dr. Chapman as to another fact, which is this: It is sometimes found after death that the spleen is not much enlarged or congested, while there is the strongest evidence to

prove such congestion, or enlargement even, as existing before death. This applies also to the liver. I will not give his explanation of this, because I do not think that it is at all more intelligible than his theory to account for the congestion during life. My explanation is that the congestion of both the spleen and liver, when they disappear *in articulo mortis* or post mortem, is to be accounted for by the giving way of the valves in the azygos veins and in all other veins having valves, thus allowing the blood to be forced back into the lungs, into the spinal column, and into the veins distributed over the surface of the body. That the veins on the surface, which a short time before were almost empty, as evidenced by the pallor and by the cold and shriveled condition of the skin, do fill up, causing the body to become warm after death, is a fact which has been observed by all physicians, who have had much to do with cholera. Now in this condition, when the action of the heart is so extremely weak that no pulse can be felt at the wrist, it is preposterous to suppose that the increased quantity of blood which is found upon the surface has passed through the heart before it reaches the skin; or that it is sent to the external parts of the body by the action of the heart, causing it to go first through the lungs and then through the systemic and other arterics. If my explanation as to the giving way of the valves does not account for the conditions referred to, then, in my humble opinion, there is no explanation to be given.

On the death of the nervous system Dr. Chapman says, "I have already mentioned the fact, on the authority of Professor Parkes, that 'two or three hours before death there is often some return of heat in the scalp and forehead, over the region of the heart or whole chest, and it may be also over the abdomen; the extremities are still icy cold, and the cholera visage is unaltered. This partial return of heat on the head and trunk is an immediate forerunner of death, and, as far as I have seen, is invariably a fatal sign; it is occasionally confined altogether to the cardiac region, and is sometimes astonishingly great.'

"This remarkable phenomenon is immediately due to the re-

laxation of the various branches of the pulmonary artery, of the bronchial tubes, and of those systemic arteries distributed over those parts of the body which, during the disease, have continued most vital, and have therefore preserved the highest temperature. The blood has continued to pass through these arteries most copiously, and is thus prepared to effect their dilatation, and to flow through them in fuller currents than before, the moment the energetic stimulus from the negative motor nerves, which has kept them in a state of tonic spasm, declines or ceases. Now already, when the phenomenon in question presents itself, the ganglionic nervous centers presiding over the arteries just mentioned have begun to die. Their convulsive grasp of the blood-vessels and air-tubes, which have already proved fatal to the system generally, is being relaxed, and they themselves are sharing the fate which, through their agency, has overtaken the entire organism. A last but vain effort for life is made, however, by the structures, released at length from the deadly influence of their excessive energy. The normal attraction between the venous blood in the pulmonary arteries and the air in the air-cells generates movement of the blood through the pulmonary capillaries; it reaches the left heart, and is thence forced most copiously into those systemic arteries just indicated, and thence, finding its way to the starving tissues, the usual vital changes occur. Meanwhile, as a result of the reserved oxidation occurring in the lungs, and in the parts supplied by the newly-relaxed systemic arteries, heat is evolved, and constitutes the phenomenon in question. But this local struggle for life is too late; its possibility depends on the presence of death in the nervous system, which, soon seizing on the brain itself, closes the scene. This local increase of temperature before death is a strong proof that the blood-vessels are healthy; that the structures are healthy, and suffer only from lack of nourishment; that the blood itself is free from poison; and that the disease is seated in the nervous system.

“The increase or long persistence of heat in the body after death, which is a well-established fact, is merely the continu-

ance and extension over the whole body, after the death of the brain, of the series of actions commenced before death, and explained in the preceding paragraph. The whole sympathetic system having ceased its functions, the arteries throughout the body relax; the small supply of blood in them is drawn through the systemic capillaries; every particle of oxygen which it can yield up combines with the surrounding structures; and while there are elements to continue these changes, the temperature of the body is raised, maintained, or prevented from declining with the rapidity usual after death from almost all other diseases. The reflux of blood throughout the body denoted by the general increase or unusual persistence of its heat, must inevitably result in lessening the bulk of any organ which may have been unduly distended with blood. Such an organ in choleraic collapse is the spleen, which is found unusually large during life, but which, after death, presents in respect to size no constant appearance."

Let the candid reader consider my explanation, and then consider Dr. Chapman's. How is it possible for the blood to flow from the right to the left side of the heart through the usual channels after the heart has almost ceased to beat, and after it has in fact ceased to move; for the flow of blood of which he speaks continues in the same manner, according to him, after death? He speaks of the blood being drawn through the systemic capillaries. I would ask, what draws it? In the case supposed, according to him, the nervous system is dead; and we know, as a matter of positive fact, that the heart has almost ceased to beat, or has ceased entirely.

It has been a question among physiologists whether any other power or force than that of the heart's action keeps up the current of the circulation through the arterial and venous systems from the mouth of the aorta to the right auricle; but I do not suppose that any physiologist ever conceived that the circulation of the blood through the capillary system could be carried on independent of the heart's action. Now after this, Dr. Chapman's theory, to wit, that this renewed circulation of the blood is a consequence of the death of the nervous system, in ac-

counting for the spleen's not being congested, he speaks of a reflux of the blood caused by or dependent upon a relaxation of the arteries. As every anatomist knows, there are no arteries between the mouths of the veins emerging from the spleen and the right side of the heart. All the blood flowing from the spleen and from all other parts of the abdominal viscera, except the kidneys, has to pass through the portal vein and the liver and hepatic veins before it reaches the vena cava, which conveys it to the right side of the heart. The hepatic artery, the only artery with which this large current of the circulation comes in contact, has nothing whatever to do with it. It has been repeatedly proved that injections will pass directly from the mouth of the portal vein as it enters the liver through its minute branches into the minute veins which go to make up the hepatic veins; so that a direct circulation is maintained between the portal vein on the one side and the hepatic veins on the other.

Now if, as I suppose, the valves of the azygos veins, of the bronchial veins, and of all other veins having valves which empty themselves into the larger veins within the cavities; now, I say, if these valves all give way, allowing the distended, ascending and descending vena cava, and other large valveless veins to unload themselves of their enormous congestion, this would allow a free, or freer current of blood to pass from the portal vein through the liver and hepatic vein into the vena cava, which was becoming less distended. No other explanation need be given of this than the laws of hydrostatics. The unloading of the distended portal vein would remove the congestion of the spleen in particular, and of the abdominal veins generally. The congestion of the liver itself would be removed to some extent in the same manner.

Dr. Chapman imagines a restored current of the circulation with a healthful or natural production of animal heat, because, according to him, the increased heat is produced by oxidation of the blood, and attributes both to the death of the nervous system. Now nothing has been more uniformly observed than the continuance of the intellectual powers, even up to death itself, in

cholera patients. The brain, I think, according to all physiologists, stands at the head of and controls the whole nervous system.

I have already given my views as to the apparent separation between the systems of organic and vegetative life, and of animal and intellectual life in cholera cases where the body seems to be almost dead, while the intellect remains clear. During the collapse of cholera, or, we may say, at the beginning of the cold stage, when the action of the heart becomes very weak, but a small quantity of blood is sent to the brain through the arteries. The veins within that cavity become congested, because there are no valves in the veins going from the brain to the descending vena cava. These veins often relieve themselves of excessive congestion by pouring out serum into the sinuses of the brain; but there is nothing in this condition of things which affects the integrity of the brain itself. Hence the continued clearness of the intellect in cholera patients, although it may be feeble.

Cholera Sicca.—The Greek word cholera means a water-spout, or the pouring out of a gutter. There can, I think, be no doubt that the Greek physicians found this word the most expressive of the conditions which amount to what modern writers have called cholera. But inasmuch as patients die from this malady who have no gushing rice-water discharges, we are forced to conclude that there is something operative which does not affect the mucous membrane of the bowels. I have said a little back that the rice-water discharges were almost pathognomonic of the disease called cholera; but cases do occur in which there is no diarrhea or rice-water discharges. Without particularizing dates, men died within five or ten minutes in Muscat, on the Persian Gulf, and in Teheran, Persia, who had no discharges from the bowels at all. This is well authenticated by medical writers of unquestionable standing. To-day, 1874, there is a very large amount of medical opinion, for it is nothing else than opinion, going to show that cholera is produced from one victim to another through the rice-water discharges, or cholera discharges, which are supposed to seep through the earth and get into wells or

springs. It is supposed that individuals who have swallowed water or food containing these animal or vegetable parasites have passed from one place to another, and that their alvine discharges have entered other springs or wells from which the persons drank who became the subjects of cholera. This is the bad-water theory. But when we know, as we certainly do know, that the most sudden and fatal cases of cholera occur without any rice-water discharges, we are forced to conclude that the cause of cholera does not act primarily upon the mucous membrane of the alimentary canal.

The Abbe Huc says, men found themselves suddenly attacked by a frightful disease, which in a moment deranged the entire organization, and changed a living man into a hideous corpse. We learn from medical writers that such was the fact at Muscat and Teheran. I have treated cholera patients during three epidemics, but I only saw one case of cholera sicca; the patient, a negro woman, lived three or four hours after the onset of the disease. There is medical testimony enough to show that the diarrhea, or rice-water discharges, are a consequence, or the second or third link in the chain of the morbid causes which produce the disease called cholera. I do not doubt that after the cholera poison, whatever it may be, has affected the system, that bad water or particular articles of food may be efficient to develop the malady. But it is not certain at all that these individuals would not have had cholera if they had not drunk the water or ate the food to which their attacks were attributed. Dr. Chapman supposes that solar heat acting upon the spinal cord, producing, as he thinks, hyperemia of the cord and of the ganglions of the sympathetic nervous system, is the efficient cause of the malady called cholera. I have shown, I think, conclusively that Dr. Chapman is entirely mistaken as to the physiological and pathological functions of the human body; but it is sufficient to say to his theory, that large armies have from time immemorial been exposed to excessive heat and all the other health-disturbing causes incident to long marches without having a single case of cholera among them.

In the late war in the United States of America, when there were from time to time over three millions of men in the field, subjected to all sorts of hardships and deprivations, there was not a single case of cholera; although the northern army, nearly five to one, was during the war in the Southern States, where the temperature was much hotter than that they were accustomed to at home; and this, too, when hundreds of thousands of them were breathing every day a miasmatic atmosphere. We are, then, forced to conclude that neither botanical spores nor animal products nor excessive heat, acting upon any part or the whole human body, is sufficient in itself to produce the terrible malady called cholera. But when we arrive at these facts, which we can not help coming to, we are not allowed to say that either of the causes referred to is inoperative, or has nothing to do with the production of the malady.

As to the acidity of the blood in cholera patients, there being some discrepancy among authors, I wrote to my very learned friend Dr. Daniel Vaughan, of Cincinnati, and I can not do better than give his entire letter.

“CINCINNATI, February 17, 1874.

“*Dear Sir:* I regret that I have found some more delay than I expected in my efforts to obtain from recent medical writings the most reliable information in regard to cholera, the various theories respecting its nature, and the facts adduced to support them.

“The late English, French, and German works which I have examined do not seem to have added much to the knowledge which existed on the subject in the medical world fifteen or twenty years ago. The theories hitherto advanced respecting the disease seem to be liable to many objections, and the most eminent medical writers appear to be wholly undecided in regard to its origin and mode of action.

“I can not find any positive statement that the blood in the victims of cholera is always acid, but I think it is so in the majority of cases. In the latest edition of his *Practice of Medicine*, Watson says that the blood is frequently acid. Reynolds, in his *Practice of Medicine*, mentions also the acidity of the blood on the authority of Dr. Garrod, who ascribes it (the acidity) to the presence of some organic acids. In Schmidt's *Jachrbuch* it is stated, as the result of an examination in certain cases, that the bodies of cholera patients decompose more

rapidly after death, and that their bones become soft. This softening of the bones could be occasioned only by the presence of an acid.

“In the French Medical Encyclopedia it is said that the vomited matter is invariably acid, but the other excrementitious matter is frequently alkaline. After death the blood is always found somewhat dark and thickened, and this condition may be justly ascribed to a want of proper *aeration*. In experimenting on animals, Dr. Brown-Sequard found that when the venous blood is injected into arteries, symptoms exactly corresponding to those of cholera are produced.

“A number of the authors which I have examined agree in stating that the blood contains urea, and some assign to it a small proportion of uric and oxalic acids. Some of their statements appear, however, to be founded, not on direct experiments, but on the fact that the action of the kidneys is suspended, and it would be thus reasonable to conclude that the ingredients of the urine should be found in the blood in a greater proportion than usual. In the few cases in which there is any urine passed it is found to contain albumen. The blood also, while it has a less disposition to coagulate than it does at other times, and sometimes coagulation is entirely suspended, yet when drawn from the body the blood is somewhat more thick than usual. This thickness of the blood is evidently occasioned by the loss of serum which passes away during the purging and vomiting.

“The doctrine which ascribes the disease mainly to the want of a proper aeration of the blood derives much support from the fact that during the prevalence of cholera the atmosphere is more free from ozone than at other times. Ozone has a far greater oxidizing power than ordinary oxygen, and the efficiency of ordinary air for respiration depends much on the small amount of ozone which it contains; yet atmospheric influences alone would not seem adequate to the propagation of the disease, and there are probably some other causes on which its diffusion depends. It has been found that in its inroads through habitable lands it travels not with the rapidity of the wind, but with the more moderate velocity of men in traveling and trading expeditions in different parts of the world.

“In the French Encyclopedia of Practical Medicine there is a vast amount of evidence introduced to prove that cholera spreads by a contagion or infection, that it passes to one individual from another previously affected, and that it is conveyed by travelers along land and by ships along waters. The author of the article (Dr. Marey) contends that it can not be transmitted to any great distance through the air. Watson, however, mentions a case in which the cholera poison passed from an infected ship to the steamer which towed her

into port, and looks upon this as an instance of aerial transmission of the seeds of the disease. Yet even in this case it is possible that the unfortunate effect may have arisen from the fact that the tow-rope may have been handled by the crew of both vessels in succession, as ropes are always coiled up by sailors in throwing them from one vessel to another. Many writers deny the contagious character of cholera on the ground that inoculation with the fluids of diseased patients have failed to produce the disease. But these inoculations or injections have never been performed on the human subject, and the experiments on animals are perhaps justly held to be inconclusive.

"Pasteur, an eminent French writer, ascribes cholera and other diseases to animal and vegetable parasites. This parasitic theory of late years has found many adherents and has given rise to the employment of antiseptics or prophylactics against disease. Grace Calvert, an English chemist, adopts the doctrine of Pasteur, and regards carbolic acid as the great prophylactic which is to arrest forever the inroads of cholera in the civilized world. In a lecture of Calvert, which I read some time ago, he mentions cases where hospital nurses had their garments fumigated with the vapors of carbolic acid, and with very few exceptions were free from all attacks of cholera. But Grace Calvert himself has been an extensive manufacturer of carbolic acid, and self-interest would tend to give him an exaggerated opinion of its utility in alleviating the sufferings of mankind.

"Of late years there has been too great a disposition to refer every disease to parasitical animalculæ, or bacteria, as they are generally called. I lately read a very elaborate article on puerperal fever, in a German periodical, in which the disease is ascribed to bacteria. Individuals, rich enough to possess fine microscopes, seem naturally desirous to claim too much for their labors of observation; but though the microscope has done much for medicine, it would be injury if it tended to keep investigators from other means of inquiry.

"I may here mention another fact that seems to favor the theory of imperfect aeration of the blood in the lungs. I allude to the great depression of temperature, especially toward the extremities. This in some cases is found as great as 17° to 19° of the centigrade thermometer, from 31° to 34° F. If we adopt the theory that heat is produced by the oxidation of the blood, the fact mentioned must show that the aeration is carried on in a very imperfect manner.

"According to the more recent views prevailing among the German physicians, cholera is transmitted through the medium of intestinal evacuations. It is accordingly contracted in water-closets, and

it is carried along by rivers and water-courses. It is also mentioned as an established fact that bare deserts are an insurmountable obstacle to the passage of cholera over them. It may be that the dryness of the air exerts some salutary action in destroying the mysterious poison. Egypt received the cholera first from the European vessels, though there had been previously much communication over the deserts with countries where the disease prevailed.

“About twenty years ago Dr. Knapp, a physician in Covington, Kentucky, and originally from Maryland, published in a large volume a new theory, or, as he called it, ‘Discovery of the Cause and Cure of Cholera.’ According to his views cholera was only a modified form scorbutis, and arose from defective nutrition. The evil he supposed the same in both diseases, but by some cause it was diverted from the skin, where it appears in scurvy, to the alimentary canal, where it appears in cholera. Though some of his ideas were good, his reasoning did not appear very logical. The prophylactics which he recommended consisted mainly of fresh vegetable diet and are diametrically opposite to those now generally recommended by physicians. The opinion of the French physicians seems favorable to the use of rich diet and animal food as a preventive of the disease. It is stated in corroboration of the excellence of the prophylactic that the poorer classes in the Old World suffer more than the rich, and it is said that of ten thousand persons connected with the Imperial Court of Russia not one was attacked with cholera at a time when it was felt with terrible severity by the serfs and poorer population of the Russian Empire.

“Few subjects have called forth more discussion than the career which this epidemic has pursued in overrunning the earth. It is generally supposed that its course is naturally westward; but this seems a mistake, as it propagates itself from India in every direction, and to the Chinese it comes from the west. It has also been a question whether cholera ever afflicted Europe before the present century. Nearly thirty years ago Mr. Roache told me that while he was connected with Transylvania University one of the medical professors, whose name I do not remember, maintained that the great plague of Athens in 409 B. C. was actually cholera. But the authorities which I have since read call it the eruptive typhoid fever. The works I have lately consulted contend that the cholera has always existed in India, but never passed the boundaries of Hindostan until 1817, when it went as far as Persia, and 1831–2, when it took a more extensive march around the world. There has been endless discussion as to the cause which operates at these periods to scourge mankind, some

ascribing it to English misrule in India, others to the assemblage of pilgrims, or to other causes.

“Your sincere friend,

“DR. J. C. DARBY.”

“DANIEL VAUGHAN.

“CINCINNATI, Feb. 19, 1874.

“*Dear Sir:* Though I wrote to you about two days ago, I deem the present note necessary to answer some inquiries in your letter of the 16th inst., which I received to-day. The latest work I could find on the practice of medicine is that of Dr. Aikin, published in London in 1872, and containing nearly 2,500 pages in both volumes. Aikin cites one authority (Wood) who maintains the blood is acid in cases of sunstroke, while he mentions another who regards the blood as weakly alkaline. I can only reconcile these conflicting statements, as well as others of the same character in regard to cholera, by supposing that much depends on the time when the analyses were made, and the way in which they were conducted. Fresh urine is almost invariably acid, but after a time becomes either neutral or alkaline in consequence of the formation of ammonia by putrefaction. Now, in cases of sunstroke, and also in cases of cholera, the blood has a great tendency to undergo putrefaction, and would thus lose any acid reaction it might have possessed after being removed from the system.

“I remain your sincere friend,

“DANIEL VAUGHAN.

“DR. J. C. DARBY.”

After the receipt of Dr. Vaughan's letters I had a conversation with a gentleman who was in St. Petersburg for five or six months during the last epidemic of cholera in that city. He said that the persons with whom he met at the hotels did not seem to be in the least concerned about the cholera although from seventy-five to one hundred persons were dying daily. He said that the upper and well-to-do classes there drink no water at all except that which is sold in all the shops, and known as mineral water. He was told that a tumblerful of water from the River Neva drunk before breakfast would have the same effect as a tumblerful of Crab Orchard salts water has in Kentucky. The lower classes, he said, drank the river water and the common spring water of the country; besides this their food was not to be compared with that of the upper classes.

CHAPTER XVI.

TREATMENT OF CHOLERA.

I think I have shown what cholera is. I think I have shown clearly and distinctly the pathological conditions which constitute it. It is a disease in which congestion of the great veins, and of all their branches in which there are no valves, is more excessive than in any other malady; greater even than in the cold stage of congestive chills. Unless this condition of things is considered and fully understood, it is altogether useless for a man to attempt to account for or to remove the difficulties which this congestion involves. All speculations as to the condition of epithelial covering of the alimentary canal; all speculations as to animal or vegetable parasites entering the body either through the stomach or lungs; all speculations as to the condition of the nervous system, amount to nothing if this venous congestion is not fully considered and clearly understood.

I have noticed the work of Dr. Chapman somewhat extensively, because I believe that the practice which he has introduced promises to do great good in the treatment of diseases. I differ from him totally as to his views on the nervous system. I believe that since the days of Magendie and Sir Charles Bell nothing has been discovered concerning the nervous system which has possessed in itself much practical value. I do not pretend to say that these discoveries have not advanced our knowledge of the nervous system; but the two things about which most has been said and written have had altogether too much importance attached to them. I allude to the supposed discovery of the vaso-motor nerves and reflex action. Since the days of Magendie and

Sir Charles Bell it has been known, if it was not known before, that when a horse kicks or an athlete knocks down his rival, that the muscles are made to contract and to expand by and through the power of certain nerves. As it was known long before their day, perhaps, that the blood-vessels are capable of dilatation and contraction; in other words, that they do dilate and contract, it could hardly, I think, be called a discovery for a physiologist to declare to the world that there were certain nerves which caused these blood-vessels to dilate and to contract. The so-called discovery of the vasomotor nerves is not, I think, made out with any clearness or distinctness.

A great deal has been said and written about reflex action. What does it amount to? Many a blooming beauty returning from the ball-room, when the deceitful moonlight caused her to suppose that a puddle of freezing water was a broad, white stone in the path, has had her silken slipper thoroughly saturated, the result of which was a pneumonia or some other equally serious malady. This was reflex action. Dr. Charles Caldwell used to say that it was sympathy; and he tried to make the class understand that the pneumonia would have followed the almost freezing of the feet if there had been no connection of flesh and blood between the parts. The nervous system may be compared to light. When the rays of the sun are admitted into a darkened chamber through a round hole or aperture, a white spot is observed upon the floor corresponding to the size of the aperture; but if you place a prismatic glass in the opening, the rays of light are separated, and you have the seven colors of the rainbow—red, orange, yellow, green, blue, indigo, and violet—in the place of the white spot on the floor. This white spot, or undivided light, may be said to represent the nervous system in animals of the lowest development. The whole nervous system in them is altogether mixed up and is indivisible; but as you advance through radiates, mollusks, articulates, and vertebrates you have little by little some sort of separation; after a while nerves which seem to control organic or vegetative life, and nerves of animal life, until you arrive at man and woman, in whom the nervous system is so wonderfully

divided and separated that the whole nervous system can be dissected by the scalpel almost without the aid of a microscope. But as the colors formed by the prismatic glass and by the rainbow so approximate and blend together that it would be hard to tell where the one begins and the other terminates, so it is in the nervous system physiologically and pathologically considered. The union of the nerves of organic and animal life, and also the union of these with the nerves of the special senses, and the union of all of them with the brain, is so intimate that you can not touch or affect any one part of the entire system without influencing more or less the whole. Sometimes a limb is crushed, and yet the skillful surgeon preserves it, and after a few months it seems to be as sound and as healthful as ever. Sometimes the surgeon has to amputate, but, notwithstanding, after a few months an artificial limb is worn with comfort and pleasure. In neither instance was there any disturbance of the nervous system. Sometimes, again, a mere scratch or accident, which did not attract attention, causes that terrible and mysterious malady called tetanus. Sometimes what we see, sometimes what we taste, sometimes what we smell, and sometimes what we hear, causes nausea and vomiting, and even syncope. How often has a fetus in utero been marked by an indelible spot, the result of mental emotion? All these are reflex actions, and they are all equally mysterious. So is it also in the use of therapeutical agents. I, in common with other physicians, use many medicines to act upon the nervous system; but it is, after all, only a judicious empiricism. When the most terrible of all the forms of nervous derangement takes place, I mean insanity, the very ablest physician is often at his wits' end.

Take away from lunatic asylums all hygienic rules, which have done so much to preserve and to restore health under all conditions; take away from them the necessary discipline and restraint; take away from them the control and the decision of the kind but firm superintendent; take away from them the loving care of affectionate and sympathizing women, and what have you left which contributes to the recovery of the patients? I answer, nothing,

or almost nothing. Black is the absence of all color, as total darkness is the entire absence of light; yet in the arts we call black a color, and it is often intermixed with red, orange, yellow, green, blue, indigo, or violet to produce shades of color; but its presence would mar the beauties of the rainbow or the colors of the prismatic glass. So in the animal economy. Something changes the healthful condition of the nervous system into a morbid one. It may be that this something produces what is called facial neuralgia. It may be that it produces epilepsy. It may be that it produces insanity. It is as yet all mystery. Neither the scalpel or the microscope has yet explained it. The same thing is true of therapeutical agents addressed to the nervous system.

Dr. Chapman believes that the spinal ice-bags will cure cholera. I do not doubt the value of it in some cases. He supposes that an impression is made by the ice upon the ganglia of the sympathetic nerves. The sympathetic nervous system is too far removed from the surface of the back along the spinal column to be affected by the ice, except so far as the ice affects the action of the heart and respiration, and the nervous system generally. This system of nerves is outside the long spinal column and may be said to be within the cavities of the abdomen and chest, going up to the brain over the longus colli muscle. It is located within the warmest parts of the human body, and in cholera patients great heat is complained of within the abdomen. That the application of bags of ice or of hot water to the spinal column does affect the action of the heart and respiration, I do not doubt. Many persons are greatly refreshed and invigorated by a cold bath. There is in them full reaction, accompanied by a glow over the surface. Others are chilled. In them reaction is very slow, and the bath does them more harm than good. All such should avoid cold baths. But this is a matter which has to be determined by trial. I have referred to Dr. Currie's cold-water practice in fevers. I have tried it in many cases, and it is of all sorts of practice the most delightful. But the cold water is here applied where the skin is burning hot and the patient has a rag-

ing fever. There is no remedy so prompt, so efficacious, and so permanent as a cold bath in such cases. But it is a different thing in individuals, sick or well, where the temperature is at the normal standard; and when the temperature is below the standard of health, then great caution must be used in the application of cold water or ice.

Dr. Chapman's practice was made known to the public some eight or ten years ago. The external application of ice to cholera patients was tried in St. Louis before he made his first publication on the subject. I believe the results were encouraging. I will speak of one cholera case in which I used cold water after having given my particular views about the treatment. There is another therapeutical agent which is supposed to act—as Dr. Chapman thinks the ice-bags act—that is, to effect the dilatation and contraction of the arteries. I refer to belladonna or atropine.

Dr. R. Saunders, of Paducah, Ky., was the first physician, I believe, to introduce this practice in the treatment of cholera. His method was by hypodermic injection.

In a learned and admirably written paper by Mrs. Mary C. Putnam, M. D., of New York City, on Belladonna and Atropine, I have not been able to discover any thing as to the therapeutical effects of the drug when administered to patients. I can only learn from the paper that the effects of the drug upon the heart's action are uncertain and irregular, seeming to depend upon the size of the dose and the way in which it is administered.

John M. Scudder, M. D., of Cincinnati, an eclectic physician, in his work on Specific Medication, page 85, says, "The specific use of belladonna is as a stimulant to the capillary contraction, especially of the nerve centers, a remedy opposed to congestion. My attention was first drawn to it by an article from Brown-Sequard, giving the results of his experiments with the drug, stating that with the microscope he had seen marked contraction of the capillaries following its use.

"It at once suggested itself to me, that if it would cause capillary contraction it would be the remedy for congestion; and I at once commenced experimenting with it in this direction. I will

recall my first marked case. A boy about eight years old, suffering from malignant rubeola. The entire surface was swollen and dusky; the eyes dull; the pupils dilated; the face expressionless; breathing labored; and wholly unconscious for forty-eight hours. The administration of belladonna alone (in small doses) was sufficient to restore consciousness, and a free circulation, with good appearance of the eruption, in twenty hours. The evidences in its favor rapidly accumulated, so that in eighteen months I used it with a feeling of almost certainty for this purpose."

Sydney Ringer, M.D., in his *Handbook of Therapeutics*, page 443, says, "The first effect of belladonna on the pulse is to increase its quickness, fullness, and force to the extent even of fifty to sixty beats in a minute. This condition of the circulation continues till the tongue and mouth become moist and clammy, when the pulse diminishes in frequency, and loses in strength."

"Dr. J. Harley considers belladonna a powerful heart-tonic, adducing in proof the power of this drug to reduce the frequency and to strengthen the beats of the heart when weakened by disease."

Dr. Waring, in his *Practical Therapeutics* (already referred to), says, "The following are the effects of a full dose of belladonna: Acceleration of the pulse from twenty to seventy beats, with a slight increase of its volume, and a considerable increase in the force of the cardiac and arterial contraction; a general diffusion of warmth throughout the cutaneous surface; a gentle throbbing or heaving sensation in the carotids," etc., etc. (Page 120.) . . .

"In all conditions and diseases in which there is depression of the sympathetic influence, such as syncope from asthenia, or shock, or the collapse of cholera, in failure of the heart's action from chloroform, or other cardiac paralyses, the subcutaneous use of sulphate of atropia in doses of $\frac{1}{100}$ to $\frac{1}{40}$ grain is, according to Dr. Harley, the appropriate and most hopeful means of resuscitation."

From all that I can learn of the therapeutical effects of belladonna and its preparations, I consider it worthy of farther trial in the treatment of cholera. But, I think, great caution and judgment will have to be used in experiments with it. If it has the effect to remove congestion, it is certainly a remedy in the right direction, for that is the thing particularly to be done. The weight of medical authority seems to be, that it excites the frequency and force of the heart's action; but such influence does not seem to be of long continuance.

Belladonna unquestionably arrests secretion in certain glands, as the mammary gland for instance. Whether it arrests secretion in *all* the glands I do not know; but if it arrests secretion in the liver, its secondary effect of arresting secretion would do more harm than its first effect, of exciting the heart's action could do good. From all that I can learn about the drug, I am inclined to think, that its best therapeutical effects will be found to follow small doses, with sufficient time between the doses. Should I ever have to treat cholera again I will try it; but I will use it in conjunction with calomel.

Knowing, as well as I do, that the "regular" medical profession have done, and are doing, more to prejudice the public mind against the use of calomel in the treatment of cholera, as well as of almost all other diseases, than the eclectics, the steam-doctors, and all other isms have been able to do. I have referred to belladonna in this connection because, if the medical readers of this book agree with me in every thing else that I have written, I do not suppose that many of them will be willing to adopt my practice in the treatment of cholera. The prejudice against it is too strong.

I think I have overturned the physiological theories, and the conclusions drawn from pathological experiments, which are the corner-stone and entire foundation of the new system of practice, which assumes to call itself scientific.

I am not willing that the sound principles, which I have so clearly explained, should be rejected because I attach very great importance to one particular medicine.

Up to this time, I say that calomel is by far the most reliable remedy in the treatment of cholera. If, however, any other remedy, or method of treatment, will do all that calomel does do in restoring the healthy functions in cholera patients, I will be among the first to acknowledge their value.

During the past year, both in Kentucky and elsewhere, fully one half of the cholera patients died. In some localities the mortality was much greater. So far as I have been able to learn the medical profession seem to prefer trying almost every thing else before calomel. As I believe they will still do it, I will refer to another method of practice.

Dr. John Mackintosh, from whose book I have made lengthy extracts, advocated bleeding in the cold stage of fevers. At page 104, vol. 1, he says, "Bleeding in the cold stage will, in a great majority of instances, cut it short; in fact, it will rarely fail in stopping the existing paroxysm, and on many occasions it has prevented a return of the disease to which the patients had been long subject, and by which they were nearly worn out. It is difficult to determine what quantity of blood it will be necessary to draw in any given case; sometimes it requires twenty-four ounces. I have known three ounces to suffice, and in one case an ounce and a half produced the full effect. The larger the orifice in the vein, the greater is the chance of arresting the disease at a small expense of blood; but in many cases the operation is attended with considerable difficulty, from the convulsive tremors which affect the whole body. I was once successful in arresting the disease by bleeding in a cold stage which had lasted twenty-six hours, but I regard this as an extreme case. The blood sometimes only trickles down the arm; and as the system is relieved the stream becomes larger and stronger, till at last it springs from the orifice, and frequently before six ounces are taken the patient will express relief from violent pain in the head and loins, and it will soon be observed that he breathes more freely. The tremors become slighter and slighter; and by the time a few more ounces are abstracted they will cease altogether, and with them will vanish the painful sensation of cold.

The pulse will be found stronger, and a gentle moisture will be observed on the body. If the patient be properly managed with respect to bed-clothes, neither the hot nor sweating stage will in general follow."

Dr. Mackintosh gives some interesting cases, to which I would call the attention of the reader.

Ordinarily it is not necessary to bleed in the cold stage of fever; but there are some cases of congestive chills, cases desperate in themselves, in which I would bleed and give the patient that chance for recovery where all other chances seem hopeless.

Ordinarily I would not bleed in cholera, but where all other remedies fail, and where it is manifest that the patient will probably die, I would give him this chance.

The main thing to be done is to remove the venous congestion. If you can effect only the beginning of this removal, renewed action of the heart and free respiration will follow.

Increased action of the heart and deep and full inspirations involve necessarily a free circulation of the blood. The generation of animal heat accompanies the restoration of these vital functions.

I think there are fewer objections to bleeding in the cold stage of cholera, and less risk to the patient, than in the cold stage of miasmatic fevers. There is in cholera less disturbance of the nervous system. It seems rather to be prostrated, i.e. in the majority of cases. The tremors, and shaking, and chattering of the teeth, which are symptoms so marked in agues, are not present in the cold or collapse stage of cholera.

I never bled but one cholera patient. It was in 1849. He was forty-five years old, and had had a severe attack of cholera in 1833. He was salivated at that time. I was his family physician for four or five years prior to 1849. The man was occasionally sick, and I noticed that whenever he was sick he appeared to be salivated, though he took no mercury, and I never gave him a dose under any circumstances. His attack of cholera was sudden and violent. The rice-water discharges were excessive, and his limbs were becoming cold rapidly. Not being willing to give

him calomel, I had to do something else. I placed his feet in a bucket of hot water containing mustard and salt, the water coming up to the calves of the legs, and after a few minutes applied a bandage around each ankle, and then divided the largest vein I could see in each foot, and then placed his feet again into the bucket of hot water. How much blood he lost I could not tell, but I bled him until he had a full, strong pulse. He was sitting in a chair during the time. Full reaction, with warmth, speedily followed. There had been no vomiting. The rice-water discharges were arrested. As soon as I had placed him in bed I gave him twenty grains of quinine in some brandy and water. I had no farther trouble with this case. He continued to drink the brandy and water. This man died in Lexington, Mo., in 1879.

The only explanation which can be given for this man's recovery is that the bleeding removed the congestion which obstructed the action of the heart and which prevented free respiration. I repeat that I think the remedy worthy of farther trial in cases which are apparently desperate. But if bleeding is tried I would recommend that my method of bleeding be adopted. It has many advantages over opening a vein in the arm. This is a remedy, however, which it will not do for a physician to prescribe. When practiced the physician should all the time keep his fingers on the pulse in the arm. If it becomes stronger and fuller, he may be certain that the loss of blood is affording relief. So soon as the pulse becomes strong and full the divided veins ought to be tied up. If, however, the pulse became weaker and more frequent after the blood began to flow the veins should be immediately tied up and the patient placed in a recumbent position.

But for the particular circumstances spoken of in this case I would have given the man calomel.

Bleeding in cholera is a method of treatment which no physician should practice unless he understands clearly what are the pathological conditions which constitute the disease; unless, in a word, he fully comprehends what venous congestion means, what it involves, and what are the healthful consequences which will follow its removal.

The steam-doctors say that they cure cholera by giving a lobelia emetic. They give the lobelia in a tea made of what they call composition powder, which is highly stimulating and very diffusible. It is well known to physicians generally that medicinal emesis will often arrest nausea and vomiting caused by sickness. I know nothing about this practice; but if by it they succeed in removing the congestion and in arousing the heart and lungs to vigorous action, it is another remedy in the right direction.

Mustard and salt emetics were used here by the regular physicians in the treatment of cholera in 1833. Many thought well of them; others said that they did very little good. Very few, if any, physicians gave mustard and salt emetics during the prevalence of cholera in 1849. I do not doubt that a prompt emetic would, in some cases, do good; but I do not now recollect ever having given one to a cholera patient. If I were to give an emetic I would follow it with a dose of calomel as soon as the stomach became quiet after vomiting. But calomel is the medicine upon which I chiefly rely in the treatment of cholera; indeed, I consider it worth all other remedies put together.

There is, perhaps, no place on the globe in which calomel has been given in the enormous quantities given here. Dr. Louis Marshall, of Virginia, but for many years a resident of Woodford County, in this state, claims to have been the first physician to give calomel in doses of from a dram to an ounce; but Dr. John Esten Cooke is the physician who has received all the credit, and all the blame, for this practice. He was practicing here in 1833, when the cholera appeared. He had treated fifty-odd patients, when he had a bone broken, which confined him to his house during the remainder of the epidemic. He published a report of these fifty-odd cases, nearly all of whom recovered. Commencing in smaller doses, he very soon gave calomel in ounce doses, which were frequently repeated. Sometimes he gave two ounces and over. The same practice was pursued by Dr. Cooke's son-in-law, Dr. Henry Hopson, about six miles from this city. This excessive use of calomel was a matter of constant discus-

sion. The writer was not living here at the time. When the cholera appeared here in 1849 Dr. Cooke's practice was again discussed, and I soon learned that there were sixteen persons living here then who had taken calomel in frequently-repeated ounce doses in 1833. I knew each of these individuals, and can testify that they enjoyed as good health as any other persons in the community. In 1865 some of them were still living here, and enjoyed good health.

There never was, in my opinion, a greater mistake or more unfounded prejudice than the supposition that calomel, given in large doses to cholera patients, is followed by bad consequences. There are many powerful drugs which physicians use in very large doses. Physicians often boast of the enormous quantities of opium, and its preparations, which they have prescribed. The same thing is true as to many narcotics and so-called anodynes. I have given, and hundreds of physicians have given, a pint of the best French brandy to delicate females who were flooding to death in child-bed, and not only with the effect of saving their lives, but also with this noticeable fact, that there was not as much evidence of intoxication as an ounce of the brandy would have produced upon them under other circumstances.

If calomel is of any use at all in the treatment of cholera, why should not the same rule apply to the dose of it which applies under other circumstances to opium, to narcotics, and anodynes, and to alcoholic stimulants? The whole system is in a torpid condition, and it requires a powerful remedy of some sort to arouse it. There are a "thousand and one" remedies to cure cholera.

After the cholera had disappeared from Cincinnati in 1866 I read the report in one of the Cincinnati papers which purported to be made by homeopathic physicians. The report said that they had treated over twelve hundred cases and had not lost a single case. Intending to be respectful to these gentlemen, I am bound to say that the only explanation which can be given of so remarkable a report is that they excluded from it all the cases which died, on the ground, perhaps, that the fatal cases were past

recovery when first seen. The same thing may be said of the "thousand and one" remedies to which I have referred.

There are all sorts of explanations as to the perfect success of every other system of practice except that of the "regular" profession, which is dubbed "allopathy."

Inasmuch as in Christendom the "regular" profession treats nine tenths of the cases, under all forms of human malady, they have constantly charged upon them all fatal cases, let the circumstances and conditions be what they may. I know that many cholera patients have recovered who were in profound collapse. Physicians of the first standing in this country and in Europe testify to the same fact. Some of these cases which recovered did not take calomel.

The question will be asked, what becomes of the calomel when given in dram doses, and in larger doses? One thing is certain, it is not absorbed. Ipecac is an emetic; aloes and rhubarb are cathartics; opium produces sleep; and calomel, I maintain, excites the secretion of bile; but why either one produces the effect which is known to follow its administration, is past finding out.

It is sixty years ago since Dr. Louis Marshall was a practicing physician. He died since the war, about ninety-five years of age. He told me that he had frequently collected the calomel and washed it after it had passed through the bowels, and then given it again.

At that time it took weeks to get goods from Philadelphia, and the economy and saving practiced then can hardly be conceived now. I do not doubt that calomel given in large doses passes through the bowels as calomel. There can be no greater mistake than to suppose that calomel given in large doses exhausts or prostrates when it is administered to patients whose vital functions are as torpid as we know them to be in collapse or approaching collapse of cholera. There are some other conditions of human maladies of a different class concerning which the same thing is true.

What I well know about the treatment of cholera is this,

that where the patients take calomel in doses of from one half dram to one and two drams they recover in a very short time, and they are rarely troubled with consecutive fever. The Kentucky physicians generally rely on calomel as the most efficient remedy in the treatment of cholera; but a majority of them prefer giving it in small doses frequently repeated. I know some eminent physicians who give one grain every five minutes. Unless there is a reliable nurse so frequent a repetition of medicine will not be attended to. The dose with many physicians is from ten to twenty grains repeated every hour or two. A larger dose, say from thirty to eighty grains, has many advantages. I have very seldom given more than eighty grains at a dose, though I would not hesitate to do so. Where there is vomiting a large dose will much more certainly arrest it than a small one. Where only a few grains are given they are easily thrown up. The weight of a large dose will cause a part of it at least to be retained.

Those who have never given calomel in large doses to cholera patients, would be astonished to see in how short a time it arrests the rice-water discharges. Among the common errors as to the medicinal uses of calomel, there is none more common than this, viz. that calomel given in large doses will run off on the bowels. Such is not the fact, even as to twenty-grain doses, not only in cholera but in other diseases; and the larger the quantity given the greater the necessity is there for giving castor oil, or something else, to work it off. Another advantage possessed by large doses is that they almost never salivate.

Again the prompt removal of all the bad symptoms seems to me to depend greatly upon the size of the dose. But the question will be asked by the intellectual general reader, why give calomel at all to cholera patients? Know not, they will say, that Dr. Hughes Bennett and others demonstrated that calomel does not cause the liver to secrete bile? Have not all the learned physiologists in the world proved that the bile when secreted and poured out into the duodenum is immediately reabsorbed, and goes back again into the blood of the portal veins? Have

they not proved that the bile is not an excrementitious substance, and that it forms no part or parcel of the feces? Have they not proved that the kidneys are the organs whose special function it is to remove effete matter from the blood? Yes, they have attempted to prove all this, and if there was one word of truth in what they teach on these subjects, it would be impossible to account for the fact, a fact about which there can be no dispute, that calomel given in decided doses does arrest all the bad symptoms of Asiatic cholera, and, I will add, cholera infantum, more promptly and certainly than any other remedy. I not only know, from personal observation in hundreds of cases, that such is the fact, but I also know that there are scores of physicians in Kentucky and other parts of the Mississippi Valley who will testify to the truth of this fact. These physicians are gentlemen of the first intelligence and highest character, and they have had abundant opportunities to know the facts.

Homeopathic medication, practiced according to Hahnemann, would make no more impression on a man in the cold stage of cholera than would a certain something make in the manuring of a ten-acre field, as President Lincoln said to the committee of homeopathic physicians who called upon him to present their claims for appointments as surgeons in the army during the war. Were Congress to appoint a corps of scientific engineers to rebuild the levees, to remove snags and other obstructions, and to open the channel of the Mississippi into the Gulf of Mexico; and were this committee to start forthwith to the Rocky Mountains, there to examine the little rivulets which go to form the first springs which are the beginnings of the mighty Missouri, as a necessary preliminary preparation for their work on the mighty torrent of the Mississippi, that corps of engineers would act about as wisely as do the physicians who expect to learn how to treat cholera by microscopic examinations of the desquamated epithelial lining of the bowels, which is washed away in the rice-water discharges; or by chemical analysis of the rice-water discharge itself.

Dr. Vaughan very correctly said, in his letter to me, that the

medical profession did not seem to know any more about cholera to-day than they did twenty or thirty years ago. The mortality remains about the same. It is because the teaching of the medical schools actually prevents men from understanding what the disease is. And when they come to propose a remedy they do not know what it is they wish to accomplish. Of course, they want to cure their patients, but how to go about it they do not know. The heart's action has to be aroused, but alcoholic stimulants will not do this. The patient must be made to take deeper and fuller inspirations, so as to convert the thick, darkened, venous blood into arterial blood; but there is no direct way to compel the lungs to act except by medicinal vomiting, unless Dr. Chapman's ice-bags will rouse to more vigorous action both the lungs and the heart. His practice, and the emetic practice, are both worthy of farther trial.

The rice-water discharges have to be arrested; but neither opium nor astringents will arrest them. The whole body, particularly the limbs, have to be warmed; but artificial heat is abhorrent to the patients, and exerts not the slightest influence. Mustard plasters only torment, and do not a particle of good.

If opium accomplishes any thing in arresting the rice-water discharges, it narcotizes the whole system, and locks up the secretions, particularly those of the kidneys and liver; and it is not possible for a man to recover from cholera until the bile begins to flow from the liver, and the urine to flow from the kidneys.

No man ever saw a cholera patient begin to improve until after the bile appeared in the alvine discharges, if there were any discharges. It matters not what sort of practice the patient has been subjected to, the fact just stated is true, and will continue to be true for all time. We are then shut up to the necessity of compelling the liver to secrete bile, if we would remove all of the difficulties just referred to. I do not believe that any medical writer denies the fact that the liver has ceased to perform this function when the rice-water discharges begin to pass off. There can be no doubt of the fact. It is not to be supposed that the liver is

diseased when it is only functionally unable to secrete bile. It is necessarily congested in the cold stage of cholera.

It is constantly said by medical men who know nothing about the liver, though they think they know every thing, and who do not really know what congestion means, that it is useless to give remedies to act upon the liver, because, as they say, there is no disease of the liver. The remark only shows their ignorance, because when the liver is really the subject of a lesion, remedies should be used which act indirectly upon it, and not remedies which act directly, as calomel, for instance. The thing particularly to be done, by compelling the liver to secrete bile, is, first and foremost, to remove the congestion of the portal veins.

As once before stated, so soon as the liver ceases to secrete bile, which is the function it is constantly at work at in health, the blood simply passes slowly through it into the hepatic veins. But reëstablish its functions, and there is at once, and immediately, a more rapid circulation of the blood through the gland; for all glands attract blood when actively secreting. So soon as this active circulation of the blood through the liver begins, the congestion of the portal veins is correspondingly removed. No physiologist can deny this. It is the enormous congestion of blood within the abdominal cavity which furnishes the vast amount of serum thrown off by the rice-water discharges. Remove the congestion, and you will arrest the rice-water discharges. Besides, the bile poured out into the duodenum is the best and most effectual astringent that was ever applied to the mucous membrane of the bowels. You husband the failing strength of the patient by arresting the rice-water discharges in this manner, but you do not do this by vesting them with opium or astringents while the liver still remains torpid; for then the condition of the blood has not been altered, and it is becoming more and more, every minute, a *materies morbi*. It is the office of the liver to separate this excrementitious substance (the bile) from the blood, just as it is the office of the lungs to separate carbonic acid from the blood. Both agencies purify it, and the arrest of either of them is fatal to life. When the locomotive moves,

every ear attached to it moves also. So when the liver begins to act vigorously the lungs also begin to act more efficiently in the performance of their functions. The healthful action of both together soon places the blood in its natural state. The heart, lying between these two organs, is soon relieved from the load which oppresses it, and the circulation of the blood soon becomes free and active, carrying warmth, by its motion, to every part of the body. The removal of the abdominal congestion, effected through the vigorous action of the liver, relieves the kidneys of the congestion which oppressed them, and they commence at once to secrete urine, thus removing from the blood other poisonous elements.

Now will calomel do all this in cholera cases? I say that it will when given in sufficient quantities; and I also say, that of all the drugs known to the physician, calomel can be given in larger doses than any other one, not only with the certainty of not doing the patient any harm, but also with the certainty of doing him great good.

When the cholera was in Lexington and in this neighborhood, in 1833, many a patient took a half pound or more of it, and yet they lived in the enjoyment of good health for many years afterward. Perhaps they took a great deal more than was necessary, but the medical profession and the world learned with what perfect safety these enormous quantities could be taken. I repeat again, that calomel given in large doses does not salivate. It simply passes off through the bowels, leaving no bad consequences. When the human body becomes mercurialized, it is the result of small doses continued for a long time.

Any one who would understand my views as to the treatment of cholera, must first read this book from the beginning, and consider well every proposition which I have discussed. I have endeavored to make each chapter intelligible in itself, but each and all of them must be clearly understood before the reader will be able to appreciate the chapter on cholera. I am entirely satisfied that I am correct as to the nature of the bile, and the functions of the liver; but whether I am or not, no medical man

denies that it is the healthful office of the liver to separate bile from the blood; and every one who knows any thing about cholera, knows that this function is suspended almost as soon as the attack of cholera can be said to have begun.

I admit that congestion, which soon follows, may possibly be removed by belladonna, judiciously given; but of this I know nothing.

I admit that the congestion can be removed by medicinal vomiting; but of this I have no personal knowledge.

Dr. Chapman says that the ice-bags remove congestion and restore the healthy functions. I have no reason to doubt the truth of his statements; but if all that he assumes for the ice-bag practice is correct, it seems to me that the practice would have been more generally adopted in London, if nowhere else.

I admit that congestion may be removed by blood-letting. I have given a single case.

No one who has read the report of post-mortem examinations of cholera subjects made by Dr. Mackintosh and others, to which I have referred, can deny that there is a profound congestion of the vena cava and its branches, and particularly of the abdominal veins which go to make up the portal circle. This congestion has to be removed by one of the methods just above referred to, or by causing the liver to secrete bile. This is, of all others, the most direct method by which the congestion can be removed.

The secretion and pouring out of bile not only relieves the congestion promptly and certainly, but it possesses this particular advantage over all other methods, viz. that it separates the effete matter from the blood, which is so rapidly increasing because of the destructive metamorphosis which is energetically at work in the cholera patient. By the removal of this effete matter—in the form of bile—every organ in the body has a purer blood furnished to it. It is a thing the beneficial effects of which it would be difficult to calculate. It is really astonishing in how short a time the whole condition of the patient is changed as soon as the bile begins to flow. How absolutely necessary, then, is it that a physician should have a correct knowledge of the functions of

the liver, and of the bile and its uses, if he would undertake to treat cholera understandingly. If he has not this knowledge it is all guess-work. It is because physicians generally have not this knowledge that there has been so great a multitude of fanciful conjectures as to the *modus operandi* of some particular remedy. It is learned nonsense, expressed in big words.

While I maintain that the secretion and pouring out of the bile relieves the congestion more certainly, more promptly, and in a more beneficial manner than any other method, I also maintain that calomel stands incomparably superior to every other drug in its power to compel the liver, or to excite the liver, to separate the bile from the blood and to pour it out into the duodenum. While this current of bile is passing into the bowels there is also a free current of purified blood passing from the liver to the vena cava through the hepatic veins. This necessitates the return of purer arterial blood to the liver through the hepatic artery. By a supply of healthy arterial blood the liver is better enabled to perform its normal functions. The heart itself is stimulated in a healthful way to more regular and vigorous action, because the blood which is returned to it through the ascending vena cava has had a large part of the effete matter removed from it by the liver. In like manner a different quality of blood is sent to the lungs through the pulmonary artery. In consequence of this the blood is more thoroughly oxygenated as it passes through the lungs. Neither the heart nor the lungs would have taken on healthy action had not the liver been compelled to separate the bile from the blood.

The removal of the abdominal congestion, as I have before said, immediately relieves the kidneys of the congestion which oppressed them, and a more healthful and freer current of arterial blood being sent to them from the left side of the heart, they at once commence to secrete urine. This general restoration of the healthy functions of the liver, of the heart, of the lungs, and of the kidneys, relieves at once and completely the morbid processes which were at work in the mucous membrane of the alimentary canal; and, added to this, we have the healthful influence

of the bile upon that surface. Saliva is not more necessary to a clean, sweet, and healthy mouth than is the presence of the bile to a healthy condition of the mucous surface, from the stomach to the anus.

It is well known to every intelligent medical man that there are no two organs which sympathize with each other so closely as the liver and the skin.

When the one is healthy, the other is healthy. Carrying out, then, this chain of effects from the liver, restored to the healthy performance of its functions, in the cholera patient we immediately observe that the skin, too, is beginning to perform its healthful functions. Correspondingly with these agreeable changes, life-restoring in themselves, we find that the congestion of the brain is also removed, and the whole nervous system is at once itself again. General, healthful warmth, not feverish heat, diffuses itself throughout the whole body, resulting from the free circulation of a current of healthy blood.

The digestive organs, one and all of them, are now in a condition to convert the food into blood. It is food alone, after the removal of the morbid symptoms, which can restore strength and lost tissue to the body.

The spleen having been relieved from the congestion which oppressed it, is now ready to pour rejuvenated blood into the portal veins, by the conversion of the food passing through it, into albumen and fibrin.

The kidneys also go to work at their allotted duty, viz. the removal of certain excrementitious parts of the food, which ought to pass off from the body through the urinary ducts.

The lymphatics, also, throughout the entire body, are again enabled to perform their normal and healthy functions.

I again repeat, that cholera patients treated in this way almost always recover without any consecutive fever. But the question may be asked, and correctly asked, have not cholera patients recovered even from the cold or collapse stage who took no calomel? I answer, that there is not a particle of doubt that they have so recovered.

Let us examine how.

The steam-doctors claim that they cure their patients by vomiting them; and the salt-and-mustard-emetic practice has been resorted to by many regular physicians.

If the reader will refer to the chapter on Emetics, he will see that it was the common practice in the tide-water region of Virginia to give an emetic so soon as the first symptoms of a chill or ague manifested themselves. The first effect of an emetic properly given is to arouse the heart's action and to cause freer respiration. These are effected by the mechanical action of vomiting alone. But very soon after the bile is vomited up, showing that the liver is certainly at work. A part of the bile poured into the duodenum certainly passes downward, that being its ordinary and natural direction.

If I were to give an emetic to a cholera patient, I would certainly give calomel afterward; but, understanding thoroughly what it is that is to be done, I can readily see that patients might, and no doubt could, recover without the calomel. It is a thing well known to all medical men, of every school, that those of them who are entitled to be called physicians have the faculty or the ability to manage their particular system better than any body else could. You may take the first regular physicians in the United States, or in Europe, who agree generally both as to the principles and practice of medicine, and yet you will find that they will treat successfully any one, or all of the most marked forms of disease after a somewhat different manner.

In cholera cases we must know and understand what it is we have to do. The manner of doing it may be somewhat varied. In the patient whom I bled it was the pouring out of the blood from the feet in the bucket of hot water which removed the congestion. The removal of this congestion enabled both the heart and lungs to perform healthfully their natural functions. The congestion of the portal circle and of the liver being of course removed, that organ (the liver) was again enabled to separate bile from the blood, which is as much its constant function as the separation of carbonic acid from the blood is the constant function

of the lungs. The bile separated in the liver and poured into the duodenum played an important part in the arrest of the rice-water discharges. The twenty grains of quinine given to this man was a permanent tonic.

As to Dr. Chapman's method, I will have something more to say in another place. To give my speculations as to the origin of cholera, I would hazard the opinion that there is an efficient cause, which is negative, and exciting causes, which are positive. The negative cause is a deficiency of ozone in the atmosphere. This deficiency of ozone is probably connected with an altered condition of the electrical state of the atmosphere. Since the discoveries of Maury as to the currents of the atmosphere, this subject has claimed the attention of many scientific men; but yet it is far from being understood.

I suppose that there may be local currents within the area of a square mile. We know that the breadth of a tornado is sometimes only a few hundred yards. Whirlwinds are sometimes only a few feet in width. Now I suppose that there may be atmospheric currents very limited in breadth which have a deficiency of ozone. Individuals breathing these currents very soon have the oxygenation of their blood arrested, and their systems immediately become susceptible to the influence of the exciting cause or causes, as improper food, bad water, or fright. The combined action of the negative and positive causes produces all that follows. There is, then, no lesion. The malady commences in functional disturbance. The blood is at once altered, and all the secretions are arrested.

Excluding poisons like prussic acid, excluding the bullet and the dagger, and other external forces, I do not suppose that any thing could be more quickly fatal than the total arrest of the oxygenation of the blood in the lungs, and of the throwing off carbonic acid from them. The individuals who died at Muscat and Teheran, Asia, in from five to ten minutes must have been subjected to the total arrest of these vital functions. They may have had to breathe currents of air totally deficient in oxygenating power. This deficiency is, of course, variable, and hence

a longer or shorter interval would elapse before different individuals would be struck down. No doubt there are many other conditions which would assist the causes already named in creating the malady. Among these are intemperance, filthy and crowded apartments, imperfect sewerage, etc.

Dr. Mackintosh discovered, by his post-mortem examinations, that a majority of the subjects presented evidences of chronic maladies, which had already weakened their systems. Of course it would be more difficult to cure such cases.

As to my personal observations and experience in the treatment of cholera, I will only speak of the epidemic which prevailed here in 1849. The first cases occurred in June, and it raged for nearly two months with considerable violence. The first case that I ever saw was at the lunatic asylum, just within the city limits, where it prevailed for some weeks before it was in the city proper. The man was in collapse, and died.

I can not say how many cases of cholera I have treated, as I kept no memorandum of them. All that I know is, that the cholera, like all other epidemics, excluded almost every other form of disease, and that I was continually going, night and day, for over six weeks. A large number of the patients attended by me lived in the city, but there was hardly a day that I did not go to the country to see a cholera patient. Having never seen a case before, and while I had every confidence in Dr. Cooke's calomel practice, I was yet not indisposed to try more mixed prescriptions.

Dr. Cartwright, of Natchez and of New Orleans, had been very successful, it was said, in the treatment of cholera. His prescription was, calomel grains xx, capsicum, quinine, and Dover's powder, ãã grains v. This was a dose. It was to be repeated every two hours until there were evident signs of improvement. I tried this prescription, except that I put thirty instead of twenty grains into the dose, and found that it relieved the patients in many cases. I was, at the time, the physician of three families in the country, to which I would refer.

In two of these families there were fifty-odd individuals, white

and black, a large majority being negroes. As they lived some distance from the town they said to me that my town patients might prevent me from giving them proper attention if the cholera appeared among them, and they therefore wanted me to put up medicines with directions for them. I had a number of powders prepared containing each five grains of capsicum, five of Dover's powder, and five of quinine, and thirty grains of calomel. These I called powders number one. Powders number two contained sixty instead of thirty grains of calomel. I directed that powders number one should be given as soon as the diarrhea commenced, and that if one dose was thrown up another should be given immediately until it was retained.

They were to be repeated every two or three hours, according to the progress of the case, which I explained to them. But if powders number one did not arrest the rice-water discharges, then they were to begin with powders number two, or sixty-grain doses of calomel, and repeat them every two hours until the patient was relieved. There was a number of cases on each of these farms, but I did not have to go to see one of them, as every case was promptly relieved by following the directions which I had given.

On the other farm there were eighty-odd negroes and the overseer's family, some half a dozen in number. I had the same powders put up, and gave the overseer directions how to use them. This farm being within three miles of the city, I visited the patients. There were twenty-six cases of cholera among the negroes, and one white man, the brother of the overseer. He went out from Lexington with the disease upon him, and I will describe his case more particularly hereafter.

It is probable that I would not have seen as many cases among the negroes had I not been visiting this man, for the overseer gave the powders as directed upon the first manifestation of the disease. Every case recovered.

There was within a quarter of a mile of the negro-quarters and overseer's house an Irish shanty. Five or six Irishmen died of cholera in that shanty. I never learned how they were treated.

I think the success of the practice on these three farms shows that Dr. Cartwright's prescription, with my addition to it, was a very good one. Of course, in treating the twenty-six negro cases on this farm, I did not confine myself to the powders referred to. I so frequently found that these powders were rejected by the stomach among my city patients that I did not very long confine myself to them. I think it is an excellent prescription where you are called to prescribe on the first appearance of the diarrhea. But where the cases are sudden and violent, and especially where there is much vomiting, it will not do. I very soon learned to put more confidence in calomel alone. It is true that I used other remedies as auxiliaries, such as camphor and mint-water, mixed with some astringent. In some cases these were of service. I very soon abandoned mustard plasters. I thought some cases were benefited by fly-blisters applied over the stomach and bowels. I tried artificial heat in one case, by making the patient, a negro man, lie on a pallet before a large wood fire. It did him no good, and he said that he never felt the heat. In all severe cases, where collapse was threatened, I commenced by giving eighty grains of calomel at a dose, and repeated it every two hours until the rice-water discharges were checked, and after that every four hours until the bile passed off from the bowels. In a large number of cases only two or three doses would have to be given before there was a decided improvement, when the intervals between the doses would be lengthened, and sometimes the quantity would be diminished.

Physicians who never tried calomel in sixty- and eighty-grain doses would be actually astonished to see how promptly even a single dose sometimes arrests the rice-water discharges; and, entertaining the views which many of them do, they would be no less astonished to see that the first thing produced, after the rice-water discharges ceased, was about a gill of thick, viscid, dark-colored bile. The bowels had been so completely emptied by the copious and gushing rice-water discharges that it would be difficult to conceive of what this dark-colored, gelatinous stool is composed if it is not bile. I have no sort of doubt of

the fact. But it will not do to stop giving calomel when discharges of this character appear. The medicine must be continued, though at longer intervals, and sometimes in smaller doses until the discharges become of a rhubarb yellow. The length of the intervals, and the size of the doses, must be determined by the physician in attendance.

Dr. Cross says, in one of his articles, that he lost a cholera case which he had no doubt would have recovered had he not discontinued the calomel too soon.

Sometimes other medicines may be combined with the calomel to keep up the action of the liver; among the best of these is the compound extract of colocynth.

Five-grain pills, composed of equal parts of the sulphate of iron (reduced to an impalpable powder), of Turkey rhubarb, of socotrine aloes, and of calomel, are a good remedy in such cases. They are somewhat astringent, while they are slowly cathartic. I give from one to three of these pills at a dose. But sometimes you have to continue to give eighty-grain doses of calomel every two or four hours for a day or longer before you see any change for the better. The case of the young white man at the farm number three was one of these.

I suppose at sometimes I may have given one hundred grains of calomel at a dose, as I did not always weigh it. I never had any reason to regret giving a large dose; but I have often, when it was too late, had to blame myself for giving too small a dose.

Never, except in one instance, did I give over a hundred grains at a dose. I will detail this case.

She was a negro woman about forty years of age. I was called to see her at 5 o'clock P. M. She was in collapse, and had been abandoned by the physician in attendance. There was no one with her but her daughter, a child some ten years of age. I gave her eighty grains of calomel. I returned again at 9 o'clock and gave her eighty grains more. I went back at 12 o'clock (midnight), and gave her another eighty-grain dose. Having now a little leisure, I remained with her until 3 o'clock A. M., and used repeated astringent injections, as the rice-water discharges

were still copious. Not being able to remain any longer, I spoke very decidedly, and told her I would give her a dose which would cure her, and gave her an ounce of calomel. When I returned, between 9 and 10 o'clock the following day, she smiled and said, "Well, doctor, that dose did cure me, for I have not had another operation since I took it." The calomel had to be worked off with castor oil. This woman was salivated, but she speedily recovered.

I know that I treated over three hundred cases during this epidemic, and hers was the only case in which the salivation was the least troublesome. I was attending on a white woman at the same time who had a uterine trouble, to whom I gave three grains of blue-mass. It was difficult to tell which of the two cases had the sorest mouth. I have no sort of doubt that the ounce of calomel saved this woman's life. I can not tell why I never tried so large a dose the second time; but I would ask, had not so large a quantity better be given when it was found that eighty or a hundred grains were not sufficient?

I have no more doubt that calomel excites the liver to action than I have that a piece of pickle will excite the salivary glands and make the mouth water; but with some persons the bare sight of the pickle will cause the saliva to flow, while others have to take a good-sized piece into their mouth.

The gospel of Jesus Christ was to the Jews a stumbling-block, and to the Greeks foolishness. The scientific medical men of the world who agree with Dr. Hughes Bennett, and who believe that the kidneys remove effete matter from the blood, will consider my teachings as foolishness; but I tell them that there is no other way to understand cholera, both as to the nature of the disease and the treatment, than as I explain it.

Were I to have cholera I would much sooner rely upon a steam-doctor, or a homeopathist, than a physician who entertains these so-called scientific notions.

The steam-doctor would vomit me, at any rate, and the homeopathist would do nothing, either of which I would prefer to hypodermic injections and other visionary experiments. Per-

haps both the steam-doctor and homeopathist would give me a plenty of cold water. Of this I will now speak.

As far back as 1833 cholera patients in this city were allowed to drink ice-water freely. From a review of Dr. Currie's work by Dr. John Esten Cooke we learn that he had commenced the use of cold water according to Currie as far back as 1804. In this review Dr. Cooke speaks of an epidemic fever which prevailed in Winchester, Virginia, in 1826, in which he used cold water, both externally and internally, as directed in Dr. Currie's book.

Dr. Cooke always allowed his fever patients to drink cold water freely; and when the cholera appeared, in 1833, he allowed his cholera patients to drink ice-water freely. I do not say that he was the originator of this cold-water practice, but hardly any physician, who was not familiar with Currie's Medical Reports, would have recommended it.

There is another very common error concerning cold water and calomel. Most people believe that if you take a few swallows of cold water after you have taken a dose of calomel, large or small, you are sure to be salivated. This is a mistake. I have taken many a twenty-grain dose of calomel, and always drank cold water whenever I wanted it, and never was salivated. Never wanting to salivate any patient, and having some respect for the popular prejudices, as every sensible man will have, I ordinarily direct my patients, to whom I have given calomel, to drink only a swallow or two of water at a time until after the medicine has acted.

But this precaution is not necessary in cholera. Salivation never occurs when the vital powers are so depressed as they are in the cold stage of cholera. It is only liable to occur after reaction is established. The physician has only to be attentive and to see that the calomel is worked off.

I have before said, that large doses of calomel often arrest all cathartic action in the bowels. In such cases you must give castor oil, or some other vegetable cathartic.

I have always allowed cholera patients to drink ice-water

freely. Good cistern water is decidedly to be preferred. I do not object to good French brandy or good whisky being mixed with the water; for while I do not know that it does any good—it certainly does not stimulate or create warmth—I do not believe that it does any harm. If the water is vomited up I do not believe that the patient is hurt thereby. So large an amount of serum is carried off in the rice-water discharges that the craving thirst seems to me to be the expression of a physical want, which water can only satisfy. No doubt a large part of the water drunk is absorbed.

Some have recommended putting salt into the water. Where it is not offensive to the palate and stomach of the patient, it would not be amiss to try it. Many rely upon injections in the treatment of cholera.

Among the tentative remedies, I think cold-water injections might be tried.

There is a way of giving an injection, or enema, by placing a gallon or half gallon bucket, four or five feet above the patient, with an India-rubber tube attached to the bottom of the bucket, while to the other end there is attached a glass or metallic tube, which passes into the bowels. By this method half a gallon or a gallon of cold water could be made to flow into the rectum without annoying the patient. It would probably healthfully remove the heat which is sometimes so annoying in the lower bowels, and might contribute to their more healthy action. But I never tried it in cholera cases.

I tried medicated injections in 1849, but never found that they did any good. As I used cold water externally applied in the case of the young man already twice referred to, I will now report his case.

As already stated I bled in one case. This young man was the only patient to whom I applied cold water externally. He was a man somewhat stoutly built, was five feet nine or ten inches high, and twenty or twenty-one years old. His complexion was sallow (not jaundiced) and his face had a somewhat puffy appearance. His temperament would have been called

lymphatic. He was rather slow in his movements, though attentive to business. This was the young man before he had the cholera.

He consulted me Monday morning, being then troubled with diarrhea. I told him he was threatened with cholera, and advised him what to do. But his employer urged him not to take the medicine, but to drink French brandy. This he did. Finding himself worse, he went to his brother's, on the farm referred to. I visited him at 4 o'clock P. M. Wednesday. I found him in the cold stage, the pulse being hardly perceptible at the wrist. The rice-water discharges were frequent, but not as large as I had seen them in some other cases. He occasionally vomited, but this, too, was not excessive. I gave him eighty grains of calomel, and ordered it to be repeated every four hours. As he wanted to drink brandy, I told his brother to give him brandy and water whenever he asked for it. I visited this young man every day, but could perceive no change in his symptoms. The pulse continued to be barely perceptible at the wrist; his limbs were icy cold, and he had a cold, clammy sweat all over his body. He continued to take the eighty-grain doses of calomel, but at long intervals after the first day. On Sunday morning I found him in the same condition. Up to this time there had not been the slightest evidence of bile in the alvine discharges. He had drunk over half a gallon of the best French brandy since Wednesday afternoon, and he had taken two or three ounces of calomel.

Sunday morning I told him and his friends that I would try what they might suppose to be an experiment if they would promise to perform their part. They faithfully did perform it. I had an open-headed whisky-barrel filled with cold water just from the pump. I had a large wood fire made. The young man's shirt was taken off, and he was carefully carried from the bed, in a horizontal position, and put on a sheet on the floor; I then dashed bucketful after bucketful of cold water over his back and limbs, and over the front of his chest, until I had emptied the barrel. The greater part of the water was thrown

over his back and limbs. His body and limbs were rubbed for a few seconds with a wet sheet. He was then removed to a dry sheet before the fire, and was there thoroughly and briskly rubbed by stout men. In the meantime a feather bed had been well prepared, and a blanket, heated before the fire, was placed upon it. The young man, being well rubbed, was put on this warm blanket, and another warm blanket was placed over him, with other bed-clothes; all of which was tucked in well around him. His position was horizontal, there being no pillow under his head. Reaction now commenced immediately, and he continued to improve without let or hindrance. Dark, viscid, gelatinous bile soon began to pass off, but it soon became of a healthy color. He took a few doses of castor oil, and some vegetable cathartic pills. He had no consecutive fever. He is now a well-to-do farmer, living near Woodford, Kentucky; and he has repeatedly said that he has been a stouter man and enjoyed better health since he had the cholera than he ever was before.

There was not the slightest evidence of salivation in this man's case. I never before or afterward saw a case of cholera which continued for half so long a time in the cold stage, whether the patient lived or died. I suppose that the cold-water douche had an electrical effect upon this man's body. The congestions of the great veins, the torpid condition of the liver and kidneys, and the excessively weakened action of the heart, had continued up to this time, over four days; but almost immediately after he was wrapped up in bed motion and warmth appeared in every part of his body, and the secretory glands at once commenced to perform their healthy functions.

Dr. Currie says, "That some sailors at Constantinople, in the frenzy of the plague, had thrown themselves into the sea, and, it is said, that on being taken out, they have recovered."

A gentleman told me that among the passengers on the steamer from San Francisco to Panama, there was a severe fever case. The weather being very hot, the man was brought on deck, after sundown, to get some fresh air. In his delirium he jumped overboard. A boat was let down as soon as possible to recover

the man, who, fortunately, could swim. It was some time, however, before they found him and brought him back to the ship. The man was then entirely rational, and recovered his health in a remarkably short time. Now I suppose that in all of these cases the cold water had an electrical effect upon the sufferers.

In my cholera case friction and hot blankets had to assist the cold douche in producing full reaction. In the other cases the temperature of their bodies was, no doubt, beyond the standard of health. But I do not suppose that the action of the cold water was in either case confined to an influence upon the nerves of motion, or the sympathetic system of nerves. I rather think that it was exerted upon the nerves, both of organic and animal life, which have their terminal branches in the skin. In the experiments with ice on cholera patients in St. Louis, I believe the ice was rubbed all over their bodies. If I were to apply ice to the spinal column in cholera cases, I would not put it into India-rubber bags, as Dr. Chapman directs, but into cotton bags, so that it might melt in almost actual contact with the skin. I think the ice would thus exert a more direct influence. The electrical current, as I conjecture, passing between the ice and the nerves of the skin would cause an irritation which might be felt in every part of the body, and by the heart itself, which would at once be stimulated to renewed action. In this cold or collapse stage of cholera I think it idle to talk about any thing being done, through the capillary system of blood-vessels, until after the congestion of the great veins had begun to be removed. The external application of cold water or of ice certainly demands more extensive trials in the treatment of cholera. But a man must first understand what cholera is, and then he should carefully study Currie's Medical Reports, as well as Dr. Chapman's Essays, before he undertakes it.

I said that I had treated over three hundred cases of cholera in 1849. I mean cases in which there could have been no dispute as to their diagnosis. I am satisfied that I treated largely over that number; and I am also satisfied that I did not lose over thirty cases which were entirely under my management. I saw

some cases in consultation which died, but I was not called to see them until they were past recovery under any treatment. It is more than probable that they would have died had I been the first physician to see them. In all cholera epidemics there will occur cases which are beyond recovery from the onset. They may not be so suddenly mortal as at Muscat and Teheran, but they are bound to die. But cases recovered in Lexington in 1833 and in 1849, and in the lunatic asylum near here in 1855, which were apparently as hopeless as the cases which terminated fatally. Indeed, many cases died which, as far as any body could tell up to a very short time before death, were not half as bad as many cases which recovered.

Dr. Mackintosh says that one of the most apparently hopeless cases he ever treated recovered. Cholera, I think, is the most manageable of all severe forms of disease. There are mild types of measles, of scarlet fever, of typhoid fever, and even of small-pox; but when we have the severe or malignant types of these fevers, they are more difficult to manage than cholera. Inasmuch as the mortality of cholera cases almost every where, and up to the present time—for it is now prevailing in Brazil—is put down at fifty per cent or higher, it can not be said that there is any mild type of cholera. I know that in the case of cholera as an epidemic, as with other epidemics, the disease becomes milder, and more manageable as it begins to disappear. But in Lexington, in 1849, there were a few suddenly fatal cases after it was thought that we would not have another.

Whatever you propose to do in cholera must be done quickly. I know that large doses of calomel will not only arrest the rice-water discharges, but that it will cause the liver to secrete bile freely and pour it out into the duodenum, thus relieving the congestion. Having thus stopped the pouring out of serum from the blood, and having set up another action which separates the effete matter from the blood, it is then all fair sailing. You have only to see to it that the liver continues to secrete bile and to pour it out. Attending to this, you have only to give your patient proper food, and with good nursing he will certainly get well.

It is now very common with many physicians to give calomel in combination with quinine. I see no objection to the practice in cholera cases, but think it would be better to postpone the quinine until after the liver has begun to pour out bile. As to quinine, much will depend upon its being agreeable to the stomach.

Up to the present time calomel is the only medicine which can be relied upon to cure cholera. I have explained how it does it. It is possible that some other medicine may be discovered which will accomplish the same good result. But it is not possible that cholera can be cured in any other manner than as I have explained. The congestion, which is the most marked of all the elements of cholera, must be removed. This can not be effected without causing the liver to act vigorously in the performance of its proper functions.

That the liver may be made to act by other indirect methods, I have already admitted. These indirect methods, then, constitute the field of discovery. It is one which demands the most careful exploration; for the prejudice against calomel is so strong and so deeply rooted in the minds of the people, that it is not probable that calomel will be given a fair trial in the treatment of cholera for a long time to come. It will not have this trial until many other microscopic and chemical theories have proved themselves to be worthless; not until the people prefer taking a dram, or more, of calomel to dying.

APPENDIX.

THE USE OF COLD WATER IN THE TREATMENT OF FEVERS.

[From St. Louis Medical and Surgical Journal, July, 1878.]

The history of medicine presents no fact so singular as that of the use of water in the cure of diseases. Dr. Currie says that its uses as a remedial agent had been so entirely forgotten that he claims his method of using it to be a discovery.

He took particular pains to introduce his book to the medical profession. He sent copies of the first edition, published in 1797, to the medical schools in Ireland, Scotland, and England, and gave copies to many surgeons of the British army and navy. These gentlemen, together, visited all parts of the habitable globe. The practice was a success wherever tried, whether in the north of Scotland or within the tropics, and yet Currie's method was again soon forgotten.

I have been practicing medicine over forty-three years, and my attention, and that of a large medical class, was called to Dr. Currie's book in an introductory lecture delivered by Dr. John E. Cooke, at Lexington, Ky., November, 1835. Dr. Cooke had adopted Currie's method in 1804, at Winchester, Va., and yet I have not met with half a dozen physicians who ever saw a copy of Dr. Currie's book; and until within the last few years as many more who knew any thing at all about his method of using cold water. The practice came back by accident when Priesnitz introduced his hydropathic system. I will give one of the accidental cases as an illustration. I could give several score of them.

A distinguished lawyer told me that he had a severe attack of miasmatic fever while stopping at the residence of a friend

near Natchez, Mississippi. Two or three of the best physicians of that city were attending on him. After he had been ill ten days or two weeks, I forget which, he overheard them say in an adjoining room that he could not live over twenty-four hours.

They had not allowed him a drop of cold water to drink, nor had it been applied to his body in any manner. That night his nurse went to sleep. He got out of bed the best he could, and crawled to a large spring in the yard. He drank a very large quantity of water, and then got into the spring and remained there until he felt comfortable. He then crawled back to bed and immediately went to sleep. The next morning he was convalescent.

I will give one other case where cold water was drunk. A gentleman told me that he was called to sit up with a young man in Garrard County, Kentucky, who was very low with fever. He sat up with the patient till two o'clock, A.M. As soon as every body else had left the room, the patient asked his nurse to go to the spring and bring a pail of fresh water. At two o'clock, when the other nurse was called, the first one said to the second, "If he wants any more water, there is a fresh pail of it in the porch." The reply was, "You have not given him cold water! His physician positively forbid it." The answer was, "I did not know that; he has drunk over a gallon." The next morning this patient was convalescent.

I could give a hundred cases where cold water was only drunk in this secret manner, which were told to me either by the individuals who had been cured by *drinking* cold water or who had been witnesses to the fact. I will make the application of these facts before I conclude. Dr. Currie's method is now referred to by many of the first physicians, both in Europe and the United States, but his method is not followed. I will now give it, and then the methods now practiced in Europe.

I copy from a reprint of the fourth London edition of his work, published at Philadelphia in 1808 (see page 27, etc.):

"*The manner in which the Affusion of Cold Water ought to be used in Fever.*—It will be proper to premise that when the term fever is used in the present work without an adjunctive it is the low, contagious fever that is meant. This is the typhus of Dr. Cullen, the contagious fever of Dr. Lind, the *febris inirritativa*

of Dr. Darwin. In popular language it is generally called the nervous fever, and, where particular symptoms appear, the putrid fever. This is the common fever in England. Who ever has watched the progress of fever must have observed the justness of the observation made by Cullen, Vogel, De Haen, and others, that even those *genera* which are denominated continued are not strictly such, but have pretty regular and distinct exacerbations and remissions in each diurnal period. In this space of time Dr. Cullen contends that an attentive observer may commonly distinguish two separate paroxysms. My observations do not enable me to confirm his position in its fullest extent, but one exacerbation and one remission in the twenty-four hours seem generally observable. The exacerbation usually occurs in the afternoon or evening, the remission toward morning.

“If the heat of the patient be at such times taken by the thermometer it will be found to have risen one or two degrees in the central parts of the body above the average heat of the fever, and still more on the extremities. The safest and most advantageous time for using the aspersion or affusion of cold water is when the exacerbation is at its height or immediately after its declination has begun; and this has led me almost always to direct it to be employed from six to nine in the evening, but it may be safely used at any time of the day *when there is no sense of chilliness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse sensible perspiration.* These particulars are of the utmost importance.

“First. If the affusion of cold water on the surface of the body be used during the cold stage of the paroxysm of fever, the respiration is nearly suspended; the pulse becomes fluttering, feeble, and of an incalculable frequency; the surface and extremities become doubly cold and shriveled, and the patient seems to struggle with the pangs of instant dissolution.

“I have no doubt from what I have observed that in such circumstances the repeated affusion of a few buckets of cold water would extinguish life. This remedy should therefore never be used when any considerable sense of chilliness is present, even though the thermometer applied to the trunk of the body should indicate a degree of heat greater than usual.

"Second. Neither ought it to be used when the heat, measured by the thermometer, is less than, or even equal to, the natural heat, though the patient should feel no degree of chilliness.

"Third. It is also necessary to abstain from the use of this remedy when the body is under profuse, sensible perspiration, and this caution is more important in proportion to the continuance of this perspiration. Sweating is always a cooling process in itself, but in bed it is often prolonged by artificial means; the body is prevented from cooling under it to the natural degree by the load of heated clothes. When the heat has been thus artificially kept up a practitioner, judging by the information of his thermometer only, may be led into error.

"Under these restrictions the cold affusion may be used at any period of the fever; but its effects will be more salutary in proportion as it is used more early. When employed in the advance stages of fever some cordial should be given immediately after it, and the best is warm wine.

"CASE I.—A nurse in the fever ward of the Infirmary was seized with violent rigors, chilliness, and wandering pains succeeded by great heat, thirst, and headache. Sixteen hours after the first attack her heat at the axilla was 103° F., her pulse 112 in the minute, and strong; her thirst great, her tongue furred, and her skin dry.

"Five gallons of salt water, of the temperature of 44° were poured over her naked body at five o'clock in the afternoon; and, after being hastily dried with towels, she was replaced in bed. When the agitation and sobbing had subsided her pulse was found to beat at the rate of ninety-six strokes in the minute, and in half an hour afterward it had fallen to eighty. The heat was reduced to 98° by the affusion, and half an hour afterward it remained stationary. The sense of heat and headache were gone, and thirst nearly gone. Six hours afterward she was found perfectly free from fever, but a good deal of debility remained. For several days the cold affusion was repeated at the same hour of the day as at first; the fever never returned. The bath was repeated, in the second case, four times; in the third case, six times; in the fourth case, four times; in the fifth case, three times; in the sixth case, three times; in the seventh case nine times."

Dr. Currie gives an account of the fever that prevailed in the Thirtieth Regiment at Liverpool in 1792: "It extended to fifty-eight persons, in all of which thirty-two went through the regular course of the fever, and in twenty-six the disease seemed to be cut short by the cold affusion; of thirty-two, already mentioned, two died. These two cases were men so debilitated by previous infirmities that it was not thought advisable to use the cold affusion at all. In some of the cases reported the cold affusion was not used until the ninth day of the fever. In a majority of the cases it was applied only once in the twenty-four hours. It was not used oftener than twice a day in any case. It was always applied at the exacerbation of the fever.

"The cold affusion may also be employed with success in intermittent fevers, as I have found by repeated trials. Though the patients were often startled at the first proposal of dashing the cold water over them, yet, after one trial, there was seldom any difficulty in persuading them to have it repeated. The effects were, in general, highly grateful and refreshing to their sensations.

"The extinction or abatement of fever was commonly followed by more or less diaphoresis, and this again by refreshing sleep. At first, I used fresh water; afterward fresh water mixed with vinegar; and lastly, a saturated solution of sea-salt in water. In the instance of the Thirtieth Regiment I used the water of the river, which contains about a thirty-third part of salt, as has been already mentioned, and this I commonly employ in private practice. I was led to prefer salt-water to fresh on account of the stimulating effect of sea-salt on the vessels of the skin, by which I apprehend the debilitating action of cold is prevented. Salt-water, either for the purpose of immersion or affusion, is more grateful to the patient than fresh water, and it is well known that it may be applied to the surface for a length of time with much less hazard. Persons immersed in sea-water, and especially in saturated brine, for some time together, preserve the luster of the eye and the ruddiness of the cheek longer than those in fresh water, of an equal temperature, and such persons exhibit the vital reaction stronger when removed from it."

The heat was always tested by the thermometer placed under the tongue or in the axilla, and the cold affusion was never applied unless the heat was above the standard of health, and not

in a single case reported unless the temperature was 101° F. or higher. The affusion was made by dashing bucketsful of cold salt-water over the patient in quick succession; and the patient was quickly rubbed dry and put in bed.

“Drinking Cold Water.—The effects of cold water as a drink in fevers I was naturally led to examine by my experience of its effects as an external application. I have made this examination with the thermometer in my hand, and with all the attention in my power; and the following results, which will save the reader the fatigue of reading the particulars of various cases and experiments, seem to me to contain all the information necessary to direct our practice. Cold water is not to be used as a drink in the cold stage of the paroxysm of fever, however urgent the thirst. If the thirst is gratified in the cold stage of the paroxysm, it ought to be with warm liquids. When the hot stage is fairly formed and the surface is dry and burning, cold water may be drunk with the utmost freedom. Frequent draughts of cold liquids at this period are highly grateful. They generally diminish the heat of the surface several degrees, and they lessen the frequency of the pulse. When they are attended with these salutary effects, sensible perspiration and sleep commonly follow.

“Throughout the hot stage of the paroxysm cold water may be safely drunk, and more freely in proportion as the heat is further advanced above the natural standard. It may even be drunk in the beginning of the sweating stage, though more sparingly. Its cautious use at this time will promote the flow of sensible perspiration, which, after it has commenced, seems often to be retarded by a fresh increase of animal heat. A draught of water taken under such circumstances will often reduce the heat to the standard at which perspiration flows more freely, and thus bring the paroxysm to a speedier issue.

“Among the ancients the internal use of cold water in ardent fevers is recommended by Hippocrates, Galen, Celsus, and most of the celebrated physicians whose works have come down to us.” Among the modern, that extraordinary man, Cardanus, wrote a dissertation in its favor, and to pass over a multitude of inferior names, Hoffman, though with some restrictions, recommends it not in fevers only, but in various other diseases. In our own country it was proposed as an almost universal remedy by Smith,

and a treatise has been written on it under the title of *Febrifugum Magnum* by Dr. Hancock.

In Spain and Italy the use of cold water in fevers obtained, in the beginning of this century, a greater and more general reputation than in any other countries in Europe, and at one time seems to have superseded all other diet, as well as medicine. This treatment was celebrated under title of *Diæta Aquæ*.

This practice was opposed by the celebrated Boerhaave. And yet Dr. Edward H. Clark, in his *Century of American Medicine*, begins his article in the words which immediately follow: "When Boerhaave, the most accomplished and celebrated physician of the eighteenth century, died, he left behind him an elegant volume, the title-page of which declared that it contained all the secrets of medicine. On opening the volume every page, except one, was blank. On that page was written, 'Keep the head cool, the feet warm, and the bowels open.' This legacy of Boerhaave to suffering humanity typified not inaptly or unjustly the acquirements, not of medical science, but of medical art, at the close of the eighteenth century."

In this article of Dr. Clark's, twenty-two pages are devoted by Dr. Bigelow to the discussion of the question as to whether Drs. Morton, Wells, or Jackson discovered modern anesthesia. While I admit the inestimable value of chloroform and ether in the practice of surgery and obstetrics, it must be admitted by every one who will allow himself to think, that one thousand, if not ten thousand, patients are comforted by a free drink of cold water to one single case where the use of chloroform or ether is admissible. The same is true to a less extent as to the application of cold water to the bodies of fever patients, according to Currie's method.

But when a Boston physician, writing in 1876, was ignorant that such a school as the Medical Department of Transylvania University ever existed, or that there was ever a medical school west of the Alleghany Mountains, it is not to be wondered at that he never knew that such a man as James Currie ever lived.

Thomas Jefferson said that he had lived long enough to outlive three different medical theories. I say that the human family will not live long enough to outlive the doctrines of Currie and his predecessors, when correctly and fully understood.

I will now briefly refer to the other methods. In his article on Typhoid Fever, Niemeyer says, "As often as the temperature rises above 104° the patient is placed in a bath whose temperature is about 10° below that of his body, or about 94° . While the body and limbs are gently rubbed we add cold water gradually till the temperature of the bath is reduced to about 68° . The patient is to remain about twenty or thirty minutes in the bath, till he is slightly chilled, and then to be placed quickly in a warm bed. At first four or five baths are necessary; subsequently two or three a day."

Dr. Brand's Method, as practiced at Stettin.—Dr. Brand endeavors to maintain the temperature of the body below fever heat at the beginning of the disease. At the first, or beginning of the second week, the patient is to be placed up to his neck in a bath at the temperature of 68° F., and at once cold water— 43° to 47° F.—is poured over his head; an important point, especially when cerebral symptoms are present. The following days, when doubtless the nervous system will be excited, water at the temperature of the bath will serve for the douche. The douche should be continued during a couple of minutes. The assistant will afterward rub and shampoo under the water the limbs of the patient during some three or four minutes; the patient is then to be left quiet. By that time a transformation can already be observed in him. He was, perhaps, unconscious when placed in the bath; he now begins to complain; the tongue gets moist, the face assumes a more natural appearance, and the previous drowsy, stupid look gives way to an expression of astonishment and fear. After the patient has been some eight minutes in the bath he is seized with a violent rigor; the teeth chatter, he pants for breath, and a violent and repeated fit of coughing is followed by the expectoration of thick bronchial mucus. Frequently there is an involuntary stool. The patient by this time feels anxious, and endeavors to get out of the bath, but the physician must here more than remember Dr. Brand's aphorism: "The patient will shiver and chatter his teeth, but he must remain fifteen minutes in the bath." If the rigor appears early, the bath should last only fifteen minutes, and somewhat longer should the shivering appear later. Before removing the patient from the bath the head-douche should be repeated. Taken out

of the bath, the patient can be supported by the assistant; he trembles with cold, his skin marbled, shaking like a leaf beaten by a strong wind. He is at once carried to his bed, and his shirt should be put on without previously wiping him; a single sheet should be thrown over in summer, or a light blanket in winter. Next, a cupful of tepid broth should be given to him, followed by a mouthful of good old wine; he is then to be left perfectly quiet. The shivering will last from twenty minutes to nearly an hour. The cold baths are repeated every third hour, day and night, until the temperature in the rectum measures only 101° F. Dr. Brand applies his method to all cases and during all the stages of the disease. Such is Dr. Brand's method.

Dr. Glenard's opinion is the following: "You will not find in the five or six thousand cases of typhoid fever treated by this method one single unsuccess among those which have been submitted to it since the beginning of the disease. To insure success Brand's method should be rigorously applied from the beginning of the disease; sometimes the treatment will require from fifty to two hundred cold baths. The duration of the disease is not lessened by this treatment; convalescence does not begin before the twenty-first day; it is, however, shorter and easier."

Liebermeister's Method. — "The appearance of Jürgensen's work, in which the results achieved at Kiel were set forth in an impartial and strictly scientific manner, marked an epoch in the treatment of typhoid fever. It appeared from this work that if the energetical withdrawal of heat from the body, was to be followed by any marked result, it must be repeated as often as the temperature of the body rose above a certain point."

"E. Brand, in Stettin, was about the only man who used cold-water treatment in the proper degree of system and vigor."

Liebermeister's method does not differ materially from that of E. Brand. He says, "As a rule in somewhat severe cases I have the temperature taken every two hours, day and night. Whenever the temperature in the rectum reaches 103°, or in the axilla 102.2°, a cold bath is given."

The success in the treatment of typhoid fever cases, recorded by these German physicians, is extraordinary. Dr. Cullen said

that medical facts were medical lies. I will not be so uncharitable; but from my observation of typhoid fever cases, and from what I know of the lesions that occur in the intestines in these cases, I can not understand how the patients could stand so severe a shock, and that continually repeated.

The practice above detailed is not at all according to Currie. I have never used the aspersion of cold water or the putting the patient into a cold bath, except in the exacerbations of fevers, whether they be miasmatic or typhoid. Dr. Currie says that the theory of Boerhaave, of lentor of the blood, put a stop to the use of cold water, whether as a drink or externally applied, throughout Europe; and that the cold-water treatment of fevers had been almost entirely forgotten, when he again introduced its use and wrote about it.

Tens of thousands of individuals have died in torture for the want of cold water as a drink, and as an external application, because of Boerhaave's theory. It is greatly to be feared, I think, that the use of cold water, as detailed above, by German and other European physicians, will again bring the practice into disuse.

I would urge upon all physicians to confine themselves to Currie's method. It appears that the duration of typhoid fever is not shortened by the external application of cold water as now practiced in Europe. If Dr. Currie is to be believed, the duration of the fever in his cases was cut short in a remarkable manner. Where calomel is given in the early stages of typhoid fever, as I have explained in a former chapter, and where cold water is given to drink and externally applied, strictly in accordance with Currie's method, as I have always done, I know that they are cut short, and that convalescence ordinarily begins on twelfth or thirteenth day.

The external application of cold water dose not remove the cause of the high temperature in these fever cases. That cause is the fever poison and the presence of a large amount of effete matter in the blood. How that is to be removed I have already explained. Dr. Currie gave calomel to his fever patients.

APPENDIX II.

CAN FEVERS BE CUT SHORT BY CURRIE'S METHOD, OR BY ANY OTHER?

When Lady Mary Wortley Montagu introduced into England from Constantinople the method there practiced to prevent the severity and mortality of smallpox, she conferred upon Western Europe a boon second only in value to the discovery of the immortal Jenner. And that was not all. This Asiatic contribution to the medical art of Europe demonstrated that the most malignant, the most disgusting and fatal of all fevers can be cut short; indeed can be made so mild as hardly to disturb the ordinary, healthy functions of the human economy.

This was done not by the use of a specific or an antidote, so called, but by the preparation of the individual to be inoculated with smallpox matter. The method of doing this was by dieting or the administration of some simple medicine. In other words, the physician saw that his subject was in a healthy condition. The testimony of medical men is almost unanimous as to the entire success of this method, and the very, very small percentage of deaths.

Indeed there are not a few physicians still living who prefer inoculation to vaccination. This is because vaccination is too often carelessly and imperfectly performed. It is well known that an individual may have caught smallpox and gone a week or ten days before vaccination, and yet the latter will arrest or so change the smallpox poison as to make the case a very mild one and cut it short.

The great Abernethy pursued a similar method in his surgical practice. He prepared his subjects before operating on them. The no less distinguished Benjamin W. Dudley, of Lexington, Ky., prepared his subjects always, before using the knife, by dieting and giving such medicines as he thought necessary. Aber-

nethy in England and Dudley in the United States established the method of the constitutional treatment of surgical diseases. Nowadays the reverse is too much the case, and many constitutional troubles are treated by surgical appliances. This is all because of the received physiological doctrines of the day.

The fever treated by Dr. Currie was, he said, the common fever of England, described by authors variously according to their theories. It is not to be supposed that there has been any change as to the essentials of fever in England since the days of Currie.

Andral, Chomel, and Louis were among the first to point out the particular lesions which distinguish typhoid fever, for instance, from other fevers. The fevers now recognized in England and on the Continent of Europe are typhus, typhoid, and relapsing fevers.

Of the causes of these fevers Dr. Alfred Hudson, of Dublin, in his lectures on the study of fever, thus speaks: "Typhus fever originates from a *contagium vivum* generated in the human body. It is communicable from one individual to another by infection; that is to say, a healthy man may contract typhus fever by going into the sick-room."

Dr. Hudson thinks that typhus fever may be generated *de novo* in overcrowded and ill-ventilated apartments. Typhoid fever can not, according to him, be caught by attending on a fever patient simply. It is generated from the fecal dejections, which must undergo decomposition and be introduced, somehow or other, into the water which we drink.

Liebermeister, in his article on Typhoid Fever, in Ziemssen's Cyclopaedia, says, "We are therefore forced to the conclusion that, besides external conditions favorable to the development of the typhoid poison, something else is necessary. Numerous facts render it more than probable that this something necessary is the specific poison itself. In other words, the poison of typhoid fever does not originate in decomposing substances, but finds in them a favorable ground for its growth and multiplication. The most convincing experiences show that typhoid fever never originates in any unusual amount of decomposing matter, nor from any circumstance favorable to decomposition, but is always preceded by the introduction of a case of the same disease."

Relapsing fever, according to Hudson, is developed from the decomposition of animal and vegetable matter found in sewers and elsewhere, with which sources of origin he associates the exposed banks of rivers.

Authors are by no means accurate or clear as to the specific origin of relapsing fever. These fevers all differ in many particulars as to their onset and course and the organs within the body particularly affected.

If Dr. Currie saw cases of each of these three forms of fever, as we must suppose he did, he does not distinguish or separate them. He claims for his practice a success which is not now accorded to it; indeed Dr. Hudson, in his comparatively recent work, says that Currie's practice has gone into disuse; at the same time he attributes very great virtue to the copious drinking of cold water in certain stages of fever.

It would appear from medical journals that the use of cold water in the treatment of fevers is to-day more practiced in Germany and perhaps some of the hospitals in Paris than any where else. It is to be feared, however, that the practice, as there carried out, will not become general. No form or method of medical practice which is practical only in hospitals can ever be of much real value to the human family. We will state this modern practice according to the most recent and highest authorities, E. Brand and Liebermeister.

According to them, an adult fever patient with a temperature of 102° F. in the axilla is to be placed in a full-length bathtub, the temperature of the water being 68°. He is kept in the tub for ten minutes, and then wiped dry and put to bed. In feeble cases the method of Ziemssen is preferred. The temperature of the water is 95° F., which is to be gradually reduced by pouring in cold water.

Liebermeister says, "Above all things, it is important for the physician to free himself from the delusion that any thing essential can be accomplished by one or by a few baths. In very severe cases it is necessary to repeat the bath every two hours, so that twelve baths are given every twenty-four hours. In some instances that have occurred in the hospital at Basle, the number of baths required by a patient during his entire illness has exceeded two hundred. These were especially obstinate attacks,

in which the intense fever would undoubtedly have caused death had any less energetic treatment been adopted."

"In the majority of cases, especially if antiphlogistic drugs are administered at the same time, four to eight baths per diem will be found sufficient, with forty to sixty in the aggregate. I would particularly insist upon the urgent necessity of continuing the baths during the night as well as by day in severe cases as often as the recurring high temperature demands."

"Cold affusions have much less effect, according to direct calorimetric observations, than baths of the same temperature and duration, but they are much pleasanter to the patient. They are, therefore, only to be recommended when the circumstances are such as to forbid the use of any more efficient method of abstracting heat; or when one desires not so much to lower the temperature as to produce a brisk, stimulating effect on the physical functions or the respiration."

It seems that Liebermeister does not push the practice as did E. Brand at Stettin, where the patients' teeth chattered and they shivered from cold. The success claimed from this practice was at most complete, and yet the fever cases (typhoid) were not cut short, but continued their usual course of twenty-one days. Convalescence was more rapid, though many of the patients had a crop of boils.

We say that this method of using cold water in fever cases is not practicable in private practice. From a careful examination of Dr. Currie's book we can not find that he used the cold water affusion in any case over nine or ten times. Dr. Currie had no theory to be sustained or explained by his practice.

High temperature is now said to be the cause of death in fevers; and it is to reduce this temperature, and keep it reduced during the entire course of the fever, that this modern German practice is founded.

J. Burdon Sanderson, M.D., etc., etc., has published in the *London Practitioner*, for 1876, three highly interesting and instructive papers on *The Process of Fever*, in which temperature is fully considered. He says, "The recognized characteristics of fever are those which relate either to the disintegration of the living substance of the body, or to the increased or diminished constancy of the bodily temperature."

“Textural Disintegration in Fever.”—Even to the superficial observer it is obvious that in fever the body wastes. But it is only by the scientific methods of measurements and comparison that we know how this waste takes place—how in fevered persons or animals the living substance of the body is disintegrated. This central attribute or characteristic (as it appears to be) of fevers, is known partly from the study of fever artificially induced in the lower animals, partly by observation of clinical cases. From the former source we obtain facts which are more definite, and therefore better fitted to serve as foundations for our knowledge.

“But the indications afforded by clinical observations, even though they may be less precise, are often of greater value, because they bring us nearer to the purpose of all pathological inquiry—the learning of the nature and cause of human ailments.

“Both in the clinical and in the experimental investigation of diseased processes, measurement without comparison is useless.

“Thus, in health, the discharge of nitrogen in the excreta is exactly counterbalanced by its introduction in food, so that it is only when this is the case that the person or animal can be said to be in a normal state of nutrition, whether the actual quantity of nitrogen which passes through the body in a given time is large or small.”

“If, in any disordered condition of health which is under investigation, it is found that the quantity of nitrogen discharged is absolutely smaller or larger (as the case may be) than in the same individual when well, we can conclude nothing from this unless it is ascertained at the same time how much nitrogen enters, in order that by balancing the one quantity against the other, we may learn whether the amount which is stored in the body is being wasted on the one hand or accumulated on the other. Or, to use the ordinary and perfectly convenient language of physiology, we judge whether integration or disintegration of living substance is going on, not by the absolute income or expenditure, but by the nitrogen exchange—by the excess or defect of nitrogen expenditure as compared with nitrogen income.”

“In health, the whole of the nitrogen discharge is derived

from food. In inanition, when nitrogen income vanishes, all the nitrogen which passes out as urea or otherwise is derived from stored or tissue albumen."

I fully concur with these views of Dr. Sanderson, except that I would conclude the last sentence by saying "is derived from effete matter."

"The general conclusion to be derived from the whole series is, that in the early stage of fever a patient excretes about three times as much urea as he would do on the same diet if he were in health; the difference between the fevered and the healthy body consisting chiefly in this, that whereas the former discharges a quantity of nitrogen equal to that taken in, the latter wastes the store of nitrogen contained in its own juices."

"Heat stands on the same line with carbonic acid, urea, and water, as a product of chemical work done in the human body."

"To determine whether or not its production is increased or diminished, we have to proceed by continuous measurement."

"The general result, to which the preceding calculation leads us, is a very important one, namely, that although, as compared with the heat-production of a fevered person is excessive, it is not by any means greater than the heat-production of health; for the highest difference indicated by the numbers stated is insignificant."

"In the first stage of fever the discharge of heat is rather diminished than increased, so that at this period it is probable that there is an abnormal retention. As the febrile process progresses, and when it is at its highest, fluctuations exhibit themselves in the quantity of heat liberated, which resemble those which are observed in the discharge of carbonic acid and of aqueous vapor; but it can not as yet be determined whether the amount of heat given off during the whole course of the febrile process is greater or less than the quantity of heat given off during the same period of asphyxia. In judging of the significance of the fact just stated, it must be borne in mind that the normal with which the febrile thermogenesis is here compared is that of inanition."

Dr. Leyden expresses his conclusions as follows: "The discharge of heat is increased in fever, whether the temperature is constant, falls, or rises. Consequently it is certain that the pro-

duction of heat is increased. In high fever the quantity of heat given off is from half as much again as the normal to twice as much. The most rapid discharge of heat takes place in the critical stage, when the temperature is rapidly sinking. It may then be twice, or even three times as great as the normal. Defervescence is associated with marked sweating and evaporation at the surface; fervescence, by the absence of these phenomena, even where the coverings are airtight.

"The question we have before us is not, whether the fact that in febrile pyrexia the temperature is, as a rule, increased both in man and animals by two or three degrees, is due to this or that cause, but simply whether in fever, along with increased temperature, there is also increased production. That *ceteris paribus* there is in fever an increased activity of *certain processes* which produces heat, can be proved without calorimetrical measurement.

"Supposing the state of the heat-discharging surfaces to remain the same, the fever patient must necessarily produce more heat to keep his body up to the higher work as regards temperature than the* healthy person. In other words, supposing the quantity and quality of clothing, and the temperature and moisture of the air to remain the same, and the discharge of watery liquid from the sweat-glands to be also the same, the fevered person would give off a somewhat larger quantity of heat from the surface than a healthy person; for the difference between the temperature of his body and that of the environment being greater, the loss by radiation and convection would be proportionally greater. The difference, however, would certainly be small, greater somewhat in summer than in winter, and greater in bed than in clothing.

"It is also certain that the process of disintegration of tissue, of which the evidence has already been presented to us, is in its nature thermogenetic. In the preceding section I have estimated that increase, and have shown that there is unquestionably a very considerable difference between the heat-production of a febrile and of a non-febrile person on the same febrile diet. I have also pointed out that this holds true only as long as the comparison is made under similar conditions of nutrition, for if we compare the heat-production of a fevered patient with that of

a person in health on ordinary diet, we find that whatever difference presents itself is quite within the limits of normal variation.

"The fact of the greater consumption of material in fever may therefore be dismissed entirely from our minds in entering on the consideration of observations similar to those of Professor Leyden.

"The temperature of the healthy human body has been recently determined by Jürgensen on a larger basis of accurate measurement than before existed. The mean temperature of a day and night of a healthy person, as measured in the rectum, is 37.2° C. (98.9° F.) If the whole period of twenty-four hours is divided into two, of which one, commencing at the moment that the bodily temperature reaches its morning maximum, ends with the attainment of the evening maximum, the other corresponding to the interval, it is found that in the diurnal period, viz. between 7 or 8 A. M. and 9 P. M., the mean temperature is 37.34° , while in the nocturnal period, of which the duration is shorter in the proportion 100 to 136, the mean temperature is 36.94° C. (98.5° F.)

"In relation to our present inquiry, Jürgensen's researches have led to a most remarkable result, which was*entirely unexpected, viz. that the mean temperature of the human body is remarkably independent of the conditions which temporarily affect the production of heat, even when their influence is most powerful. Thus, by observations on a vigorous and healthy person, who voluntarily submitted to inanition for sixty-three hours, it was found not only that the mean temperature of the two days was exactly the same as in ordinary conditions of nutrition, but that the diurnal temperature curve was not modified in its character. Again, it was ascertained by most accurate observations on the same patient, who submitted to a succession of cold baths, each lasting twenty-five minutes, at temperatures varying from 9° to 11° C. (50° F.) that notwithstanding the rapid abstraction of heat which gave rise to a shivering lasting for several hours, the diminution of bodily temperature which occurred during the bath was followed, after an interval of four or five hours, by an elevation which precisely compensated it, so that if the average was taken of observations extending over sufficiently long period the mean was the same as under normal conditions."

“The extensions of Jürgensen’s observations to fever led him to the remarkable discovery that in continued fever, and particularly in typhoid, the diurnal variation of bodily temperature closely resembles that of health.” . . .

“Clinical observation has established the fact (whatever may be its therapeutical value) that in typhoid and other fevers, the temperature can be brought down, and kept down, nearly to the normal by the systematic use of cold bath or of cold affusion.”

A candid consideration of all that I have quoted from Dr. Sanderson ought, I think, to convince any one that while the temperature in fevers may be kept down by a frequent repetition of cold baths, yet that the practice as carried out by E. Brand, particularly, is neither necessary nor advisable. It has been seen that the temperature rises after every bath, let them be repeated a dozen or more times a day, so as to keep up a diurnal equilibrium, as is the normal rule in health, and even in fever cases, where no cold baths are given.

When a bath is given twelve times a day, the temperature is made to fall and then to rise twenty-four times. Can such interruption of natural processes be advisable? It will be seen in my first article that Dr. Currie used the cold affusion at the exacerbation of the fever, which is ordinarily near the close of the day. That is the time or hour at which, in health, the regular diurnal temperature is highest. If he repeats the affusion in twenty-four hours, it is at the time of the ordinary second diurnal elevation of temperature, with which a fever case very nearly corresponds. This diurnal rise and fall of temperature does not belong alone to animal bodies. From noon until 2 or 3 o’clock the temperature of our atmosphere is ordinarily at its highest; and at or about daybreak is the coldest hour of the twenty-four. We believe that this diurnal variation in the temperature of our atmosphere, as also the diurnal variation of temperature in animal bodies, is influenced and controlled by electrical forces. We can not change the law of these forces in an animal body any more than we can change it as to the temperature of the earth.

I do not think that the cold-water treatment of fevers now pursued in Germany will ever become general; indeed I fear that very few physicians will ever give it a trial; but knowing as I do, after forty three-years’ experience, the great value of cold

water in the treatment of fevers when practiced according to Currie's method, I would urge upon every physician to give Dr. Currie's method a fair trial. Where a bath-tub can not be obtained, I have a large washing-tub filled half full of fresh cold water, in which the patient sits, and then cold water is poured from a pitcher over the back, chest, and limbs. The pouring is continued until the temperature is reduced. I have had to repeat it as often as four times within twenty-four hours; but in a large majority of cases twice a day was sufficient, sometimes once sufficed. The reduction of temperature in fever cases is not all that ought to be looked to. So soon as a fever is developed or begins to be developed, waste or the disintegration of the living tissues commences. The elimination of this disintegration (effete matter) is even of higher consideration than the reduction of temperature. Cold baths or cold affusions, carried out according to Currie's method, put the skin in a more healthy condition, and thus enable it to throw off a larger amount of carbonic acid; so far and to that extent assisting the lungs in the process of purifying the blood by throwing off effete matter in the form of carbonic acid.

There is another elimination of even greater consequence, as it is more under our control. I here refer to the action of the liver in the process of converting the effete matter brought to it along with the volume of blood through the portal vein into bile.

I will quote Liebermeister as to the use of calomel in typhoid fever. Of quinine he says, "The admirable investigations of Binz did well nigh establish the claim of quinine to be considered as a specific in all infectious diseases. Except in malarial diseases, however, it appears that this influence, if indeed it does exist, can not be obtained short of doses that may not be used with the human family. My own experience, at least after treating more than fifteen hundred cases with quinine in doses that would formerly have been considered as dangerous to life, gives no result that would indicate any specific influence of this drug over typhoid fever, nor any power to cut the fever short at any stage." My own experience fully accords with this last quotation.

As to calomel in typhoid fever he says, "Since the favorable effects of calomel on the large majority of cases was demon-

strated, I have given this medicine, with but few exceptions, to every case of the fever, admitted before the ninth day of the disease. I usually give three or four eight-grain doses during the first twenty-four hours. After having employed this method now on about eight hundred patients, I still feel that I have every reason to continue it and to recommend it to others."

Liebermeister details in a careful manner his observations, gives statistical tables of comparison between iodine and calomel, and the non-specific treatment; the result of which is, that under the calomel treatment the mortality was less than by any other treatment; and also, that under the use of calomel a majority of typhoid-fever cases was cut short, many of them not extending beyond the eleventh day, when convalescence began.

My own experience confirms the truth of Liebermeister's observations. I found, however, that whatever was to be done, as to cutting short the fever, must be done within the first week or ten days.

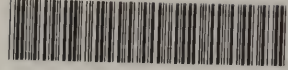
In preparing an individual for inoculation with smallpox matter we have this advantage, that we have our subject in a healthy condition. In fever cases the first thing we know is that the mischief has begun. Now I claim that we can prepare the system of a patient within the first week, so that the fever will be cut short.

In typhoid-fever cases I use calomel in conjunction with cold water. My practice has been to give five grains of calomel every six hours until the liver acts efficiently—as will appear from the bile which passes off with the stools. If diarrhea occurs, as frequently happens in typhoid-fever cases, then I combine five grains of Dover's powder with the calomel. This will arrest the diarrhea and promote the secretion and excretion of bile. Under this treatment, combined with that of Dr. Currie, as already detailed, I maintain that typhoid fever cases may be cut short and made to begin to convalesce by the twelfth or thirteenth day. A few obstinate cases may continue for a longer time. In fevers of a miasmatic origin, whether intermittent or remittent, may be cut short in a much shorter time.

In the latter case, I use quinine after the fever has been cut short. This I do because all miasmatic fevers or ailments are periodical, and apt to return.

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